SCIENCE.

FRIDAY, AUGUST 24, 1883.

THE LESSONS OF THE MEETING.

THE question as to the distance from the eastern seaboard to which the American association for the advancement of science can carry its annual assemblages is partly solved by the meeting at Minneapolis. That has registered about three hundred members in attendance; a small number, indeed, as compared with the Boston and Montreal meetings, but larger than was at first anticipated. One-third came from the Atlantic and New-England states. Astronomy and physics are fairly represented in the list; geology, as was expected, claimed the largest proportion; of botanists, there were over twenty-five — this was a surprise; the ethnologists were in considerable force; in all other branches of science, the attendance was somewhat meagre.

This, therefore, has not been one of the large meetings. Its addresses and papers have not contained any very striking feature that appealed to the interest of the general public. On the other hand, all that was presented, with few exceptions, though not brilliant, was above mediocrity. Looking over the list of papers, we find fewer than usual of the kind that brings sorrow to the hearts of scientific students; that provokes the question, How did such things ever pass the standing and sectional committees?

The merits and the disadvantages of the present system of conducting these meetings have been placed in very sharp light. Excellent addresses were delivered by most of the presidents of sections; in fact, these productions this year are a credit to the association. But the strain of obtaining such representative addresses from so many sections will soon be apparent: it may prove difficult to find the men to deliver them, within a very few years, especially if the number of sections continues to increase. The delivery of two or more of these addresses simultaneously, and the completion of the delivery of all of them in one afternoon, was felt to be a matter of grave injustice, both to speakers and hearers. To our readers we shall offer the only remedy now possible for this injustice, by printing the addresses in full, and by detachments.

Local committees, in cities to which the association will hereafter be invited, may learn some valuable lessons from the experience at Minneapolis. There was no lack of hospitable intention: the hearty courtesies of a western community were liberally extended. But the generous intentions were not carried out in the minor details that are essential to comfort if not to success. The meetings were held at a distance from the city, at the extreme end of a one-horse car route. Consequently the conveyances were overcrowded, much time was lost in going and coming, and — worse than all — few of the citizens of Minneapolis attended the sessions. We do not remember a meeting of the association at which the local interest, so far as audiences indicate it, was so deficient. The hotel selected for headquarters was not agreeable, because not exactly suitable. Members scattered to distant points, finding delicious havens of rest and recreation at summer-hotels on the lakes, but having to take yet longer time to attend the daily sessions. Free railroad transportation was provided to these distant resorts, but there was a confusing uncertainty about late trains that caused many embarrassments. These things may be trifles, but they are apt to be remembered when the lavishness of entertainment is forgotten.

As was anticipated, the association has chosen Philadelphia for its next session, where we may look again for the great numbers which attended the Boston and Montreal meetings. The exact date for holding it has been wisely left in the hands of the executive board,

pending the choice of time by the British association for their Montreal meeting. A preference, however, has been indicated for the week beginning Sept. 3, — a date earlier than usual, but welcome to all who know how warm Philadelphia can be in August.

W. C. W.

RELIABILITY OF THE EVIDENCE OBTAINED IN THE STUDY OF CONTAGIA.

THERE is certainly a disposition, among some of our scientific men, to doubt the possibility of making direct and satisfactory demonstrations of the *rôle* played by the schizophytes, or microbia, in the production of disease, and that which they may be compelled to take in its prevention. Recent publications by accepted authorities have tended rather to confirm these doubts than to remove them, and we are frequently asked if our results are not founded on probabilities rather than on definite and conclusive facts. While this uncertainty is still felt, it is well to occasionally review the connection between the facts established and the conclusions drawn from them.

Though the schizophytes are the smallest of living organisms, that is not an insurmountable obstacle to their careful study, as is proved by the well-known investigations of the Bacillus anthracis by Koch. His demonstration that this exists in two forms (a vegetating filament and a spore), and that the latter survives unfavorable conditions which destroy the former, enabled him to trace a connection between the activity of the virus and the life of the parasite, which other investigators had failed to establish. Thus, the blood of anthrax victims, which contained only Bacillus rods, lost its power to reproduce the disease after a few days' putrefaction; while that which contained spores remained virulent an indefinite time. A certain degree of cold, and also an insufficient supply of oxygen, prevent the formation of spores; and, the filaments being short-lived, the organism loses its vitality in a few days under such conditions. If spores had formed before the liquid was exposed to these conditions, however, they were unaffected, and were capable of germination after weeks or months. Again: if a virulent liquid was largely diluted, the filaments were destroyed, but the spores survived. In all these cases the activity of the virus disappeared with the death of the organism, and was retained whenever the formation of spores had enabled this to resist the unfavorable conditions.

Here was a proof of the pathogenic character of the schizophyte much more satisfactory than the mere demonstration of its presence in all cases of the disease, or the additional evidence that it might be passed through a certain number of cultivation-flasks; the liquid in the last being as virulent as in the first.

Since Koch's paper was published, Pasteur has added observations of an equally convincing character. The liquid part of the virus may be freed from the organism either by filtering through plaster or by decanting after it has stood in a constant temperature for a few days to allow the germs to gravitate to the bottom of the flask. In either case the liquid is harmless, and the separated germs still produce the disease. Again: compressed oxygen destroys the filaments, but does not affect the spores; and a virus containing only the former loses its activity when treated with this agent, while one in which spores have formed retains its virulence.

We are able to say, therefore, that, in the disease known by the French as charbon and by the English as anthrax, no liquid is virulent unless it contains the living Bacillus anthracis, and that the death of this organism always coincides with the destruction of the virulence.

This demonstration of the pathogenic action of the Bacillus cannot but be regarded as equally satisfactory with what is obtained by investigations in other departments of biological science. If the observations of these gentlemen are accurate, and they have been confirmed too often to be doubted, then there is no escaping the conclusion that the Bacillus anthracis is the essential and only cause of anthrax.

It is not to be denied, however, that the size of the parasite in anthrax, and the fact of its existence under two forms having such unequal resistance to unfavorable conditions, were characters which greatly facilitated the demonstration of its pathogenic relation to the disease. Is it possible to obtain equally satisfactory evidence in regard to the smallest of the schizophytes, and one which only exists in the vegetating condition?

The micrococcus of chicken-cholera is of this kind, and it is consequently very interesting to see just what progress we have made in demonstrating its identity with the virulent principle. We know from Pasteur's investigations that it is always present in this disease; that it may be cultivated, and passed from flask to flask for many times, without losing its virulence. The filtered liquid loses its activity; that from which the germs are

separated by gravitation is equally harmless. Taking up the study here, I have proved that the exact degree of heat which, in a given time, kills the micrococcus (132° F. for 15 minutes), destroys the virulence at precisely the same point; also that the proportion of carbolic acid, of sulphuric acid, and of a solution of chlorides (Platt's), which destroys the virulence in from two to four hours, corresponds with the proportion which is required to kill the organism in the same time.

The effect of heat and of these disinfectants on the virus was determined by inoculation experiments. The point at which the micrococcus is killed was learned by placing a drop or two of virus in the sterilized liquid of a cultivation-tube after the proper proportion of disinfectant had been added. In a given time a drop was taken from this tube, and placed in a second one which contained a favorable medium for the growth of the germs. If the schizophytes had been destroyed by the disinfectant, there would be no multiplication; while, if they had resisted it, they would certainly reveal the fact by developing in their usual manner. The exact correspondence which exists between the results of the two series of experiments in every case, is also an evidence of the reliability of the method.

While it might be conceived, that, even though the virulent agent consisted of something entirely different from the micrococcus, both might be destroyed by the same degree of heat in the same time, it is not conceivable that this would also occur from the effect of three different chemical agents. If it were necessary, this line of evidence could probably be increased indefinitely; but it is already equal to what is usually considered necessary to demonstrate a point in other departments of science.

It is possible, then, by present methods of research, to determine satisfactorily whether a given organism is the cause of a certain disease, or whether it is an epi-phenomenon; and, if there is still much doubt in regard to some of these, it would seem to be owing to the fact that observers have relied too implicitly upon the microscope, and neglected the cultivation and inoculation experiments, that are essential to definite and reliable conclusions.

D. E. SALMON.

SPONGE-CULTURE IN FLORIDA.

THE U. S. national museum has lately received from Messrs. McKesson and Robbins, sponge-importers of New York, an interesting contribution representing the first successful attempts at sponge-cultivation on the American coast. It consists of only four specimens, all of the finest or sheep's-wool variety, which were raised from cuttings at Key West, Fla., by the agent of the above-named firm. The localities in which the sponges were planted were not the most favorable for sponge-development, and their growth was therefore less rapid and perfect than might otherwise have been the case. They were fastened to the bottom, in a depth of two feet and a half, by means of wires or sticks running through them, and allowed to remain down a period of about six months before they were taken up. Fully four months elapsed before they recovered from the injury done them in the cutting, which removes the outer 'skin' along the edges of the section; and the actual growth exhibited was for about two months only. The original height of each of the cuttings was about two inches and a half. One was planted in a cove or bight where there was little or no current, and its increase in size was very slight. The other specimens were placed in tide-ways, and have grown to from four to six times their former bulk, which certainly promises well for the future of sponge-propagation. Two hundred and sixteen specimens in all were planted at the same date, and, at the last accounts, those which remained were doing finely.

The chief obstacle to the artificial cultivation of sponges at Key West arises from the fact that the sponge-fishermen infest every part of the region where sponges are likely to grow, and there is no legal protection for the would-be culturist against intruders. The enactment of judicious laws bearing upon this subject by the state of Florida, or the granting of special privileges conferring the right to occupy certain prescribed areas for sponge-propagation, would undoubtedly tend to increase the annual production of this important fishery, which has remained at a standstill for several years past, mainly because of the partial exhaustion of several of the most extensive sponging-areas.

Accompanying these artificial growths was a collection of over a hundred specimens of the various grades of Florida sponges of different sizes, each labelled with its supposed age, based upon estimates of the average rate of growth, by the sponge-collectors. This entire collection now forms a part of the American exhibit at the great London fisheries exhibition.

R. RATHBUN.

THE CONDITIONS NECESSARY FOR THE SENSATION OF LIGHT.

It is generally assumed that the only condition necessary for the production of the sensation of light by the action of radiant energy is, that the radiant energy must be of a certain wave-length within the limits of wave-length of the visible spectrum, namely, between wave-lengths 7.604×10^{-5} centimetres and 3.933×10^{-5} centimetres; that, when the eye perceives nothing, none of these wave-lengths can be present. It is worth while, therefore, to examine those physical conditions that result in giving the sensation of light to ascertain whether such assumption is warranted. As to the eye itself, it will not make any difference so far as this question is concerned, whether one accepts the Young-Helmholtz theory of vision, the Herring theory, or any other. The only important fact is, that, in either, *energy* is required and is expended in the eye; but it is important to know how to measure the energy, and to have a tolerably clear idea about its form. Without any question, a ray of radiant energy, such as is emitted by a heated molecule or atom of hydrogen, consists of a single line of undulations of a definite wave-length, for the molecule cools (that is, loses its heat-energy) by imparting it to the ether; and a 'wave-length' is simply the distance to which such a disturbance in the ether will be propagated during the time of a single vibration of the molecule. As each vibration of the latter imparts some of its energy to the moving ether, it follows that the energy of a ray of light must depend upon the number of vibrations per second; or, what is the same thing, the energy of the ray is proportional to its length. As all rays move with the same velocity in the ether, it follows that any object that should receive such radiant energy would receive an amount proportional to the time.

Suppose, now, that an atom of hydrogen be made to vibrate, no matter how, so as to give a wave-length $C = 6.562 \times 10^{-5}$ centimetres. If such a ray falls upon the eye, it will produce the sensation of redness, and, if the eye receives the vibrations for one second, it will receive 4.577×10^{14} vibrations; that is to say, it will receive as many undulations from the ether as the generating atom made in the interval of one second. Now, we know experimentally that the eye can perceive when the interval is as small as the millionth of a second, when the number of vibrations of such a ray as the above would be 4.577×10^{8}, a very respectable number. It would seem probable that that number might be considerably reduced, and still leave a sufficient number to affect the eye. If the time-interval should be made so short as the one ten-billionth of a second, there would then be 45,770 such undulations that would enter the eye. But there must be a limit to the number needed to produce the sensation; and it is also probable that this limit will differ in different persons. Admitting this time-limit, it follows that undulations of proper wave-length may exist about us, and yet not be sufficient in time-quantity to affect the eye. If other vertebrates or insects possess a shorter limit than man, it is certain that they will see when man cannot. But the energy of vibrations varies as the square of the amplitude; and hence, if one of two rays of equal length has a greater amplitude than the other, the latter might be seen, while the former might not, although they had the same wave-length.

According to the kinetic theory of gases, the molecules are in incessant motion, in which collisions result in changing the directions of the free paths of each of the molecules, and also in making each to vibrate, because molecules are elastic. This vibratory motion proper, being a change of form of the molecule, is what constitutes its heat-energy. The interval between encounters gives opportunity to each molecule to vibrate in its own periodic time or some of its harmonics. Maxwell computed the number of impacts per second for several gases,[1] and gives, for hydrogen, $17,750 \times 10^{6}$. If, then, we divide the number of vibrations per second by the number of impacts, we shall have the number of vibrations between impacts: $\dfrac{4.577 \times 10^{8}}{177.50 \times 10^{6}} = 25,700.$ This is on the supposition that the vibrations produced are all of the wave-length of the C hydrogen-line.

It is highly probable that this hydrogen-line is not due to the fundamental vibrations of the hydrogen molecule, but that it is some harmonic (the twentieth, according to Stoney). Whatever its harmonic relation may be, it must be highly probable that it will frequently be produced when the conditions are as they are in ordinary gas; but, in normal conditions as to temperature, that gas is not luminous. If this reasoning be right, the reason it is not luminous at ordinary temperatures and pressures is due solely to the slight amplitude of the vibrations of proper wave-length, not to their entire absence. When the gas is heat-

[1] *Nature*, Sept. 25, 1873.

ed, or is impelled with great energy from the terminal of an induction-coil in a Geisler's tube, it is not necessary to assume that the molecules are made to vibrate in wholly new periods, but that the amplitude of their vibrations in any and all periods has been increased, thereby giving greater amplitude, and consequent energy, to the radiant undulations emitted, sufficient to affect the eye.

When one considers the kinetic energy of molecules due to their temperature, it seems probable that all bodies — solid and liquid, as well as gaseous — must be vibrating in all possible periods continuously; but in solid sand in liquids the shortness of the free paths makes interference too frequent to allow any molecule to vibrate many times between impacts, and hence the harmonics suffer most, and are destroyed before they can have given rise to undulations in sufficient number or in amplitude to perform any optical service. By heating a solid, greater amplitude is given to all the vibrations, and we see the red or longer undulations first during the process of heating, because such are less easily destroyed by impact than the shorter ones, which cannot have at best so great an amplitude. This statement assumes that it is with molecules as it is with visible masses of matter: the greater the number of vibrations possible to it, the less the possible amplitude.

With these conditions as stated, it is readily seen why common objects are not at all times visible, that is to say, are not luminous. It is because our eyes are not sensitive enough to respond to the slight energy of the undulations due to both lack of amplitude and shortness of the rays, not because those rays are absolutely wanting. A. E. Dolbear.

RADIOMETERS WITH CURVED VANES.

Among the radiometers in a collection which I have recently examined were two with curved vanes of silver. The radius of curvature was less than 2 cm. When placed in front of a lamp, the concave side moves towards the source of heat. I have found no satisfactory explanation of these movements. According to a recent article by Dr. Pringsheim, the convex side of these vanes is supposed to be at a higher temperature than the concave side. The grounds for such an hypothesis are not obvious; and it would seem hardly possible that an appreciable difference could exist between the surfaces of a thin sheet of silver.

It is more probable that the air on one side of the vane is hotter than that on the other. Since the 'kick' of a molecule depends on its increase in temperature, the vane will move towards the side on which the air is the warmer.

Dr. Pringsheim mentions an experiment in which he brought the heat to a focus inside the radiometer at a point in front of the vane. He found that the air gave no evidence of being heated. I repeated the experiment with solar heat, using a lens of three inches diameter and four inches focal length. The heat in air was sufficient to ignite instantly a common parlor match. When the focus was kept in front of the vane of an ordinary radiometer for two minutes, no appreciable effect was observed: the instant it touched the vanes, however, they gave a start, and began to revolve. This experiment shows that the effects observed with curved vanes cannot be attributed to concentration of heat-rays from the vanes.

According to the kinetic theory, this rotation is set up only if the molecules arriving on the convex side of the vane receive a greater positive increment to their velocity than those arriving on the concave side. These conditions are satisfied in this way: if the vanes are warmer than the air, the particles leaving the vane in both directions have an increased velocity; but take, for instance, the particles moving in lines parallel to the axis of the concavity towards the vane from either side, those on the convex side are scattered by reflection, those on the concave side are brought to a focus at a distance (in this instrument) of less than 1 cm. from the vertex of the concavity. The molecules in the vicinity of this focus receive an increase of kinetic energy; and similar reasoning holds for the sets of molecules moving parallel to each other in any other direction. Hence the molecules on the concave side are hotter than those on the convex side, though not necessarily so hot as the vane itself. Since the molecules on the concave side receive a smaller increase of velocity from the vane, they give it a smaller reactive push.

The action of the case in a radiometer is very prettily shown by wetting it with cold water. The action is best examined with curved vanes, or with vanes of metal covered on one side with mica. The rotation is at first in the same direction as on heating, showing that the air has become cooled by contact with the glass, but is after a time reversed, showing, that, by quasi-conduction through the air, the vanes have become cool, while the glass is regaining its original temperature.

George W. Evans.

THE IGLOO OF THE INNUIT.[1]—II.

AMONG the natives of North Hudson's Bay, the first huts of the season, if there is a scarcity of compact snow, are made of ice. Rectangular slabs, three to four by six or six and one-half feet, are cut from some neighboring fresh-water lake where the ice has formed to a thickness of six inches. As a rough approximation, these slabs may be said to be about the size of an ordinary door. The slabs are placed almost upright, resting on their ends, and joined so as to form a circular pen of from ten to fifteen feet in diameter. Over the top of this the summer seal-skin tent (toó-pik) is spread for a roof; being supported by the tent-poles crossing at convenient places, and held in place by a lashing of seal-skin about a foot below the top of the ice-slabs.

In one of the slabs, generally on the side facing the south, a large opening is cut, which is further protected by a smaller storm-igloo having an entrance-hole no larger than the girth of the most corpulent Innuit of that particular village.

As an aid in cutting, a rectangle is marked on the surface of the ice, having a width equal to the length of the proposed slabs, and from it they are cut with an ice-chisel (toó-oke). This chisel is generally a heavy mortising-chisel, securely lashed to the end of a pole from six to seven feet long. I have seen bayonets, sabre or sword points, or sharpened files made to serve the same purpose. The Esquimaux around King William's Land used the spikes from the wrecked ships of Sir John Franklin's ill-fated expedition. The large ice-slabs weigh nearly half a ton. When dragged from the lake, they are turned on edge, and a hole cut through their centre. By means of a strong seal-skin line passed through this hole, two strong men can handle a slab with considerable ease, moving or sliding it long distances. It takes four or five persons to put the first two together, the slight inclination which is given them holding them up when once in position. After this, two or three are all that are needed to add each slab, until the house is completed. When two slabs are abutted against each other, the edges are trimmed with a snow-knife to give as much bearing-surface as possible; and, when permanently set, snow dipped in water is applied to the joint inside and out, completely closing all crevices, and, when frozen, binding the two as solidly as if but one. A handful is also put in the central hole, which held the seal-skin thong, and the ice-pen is practically air-tight around its sides. The floor of snow has become packed by the treading of the builders; and over it are laid flat stones, on which are spread a great many coarse robes of reindeer, musk-ox, and polar-bear skins, and over these the finer reindeer-skins that make the bed, which occupies over half the floor.

MAKING AN ICE-IGLOO.

These ice-igloos are as transparent as glass; and before they are covered by the drifting snow, or their interiors dimmed by the smoking of the sooty lamps, a night-scene in one of these villages, especially if it be large, with the brilliant burning stone lamps in full blaze, is one of the most beautiful sights I have ever witnessed, especially in this dreary land. Could one imagine the little Lilliputs living in flat candy-jars with drumhead covers, he would

[1] Continued from No. 28.

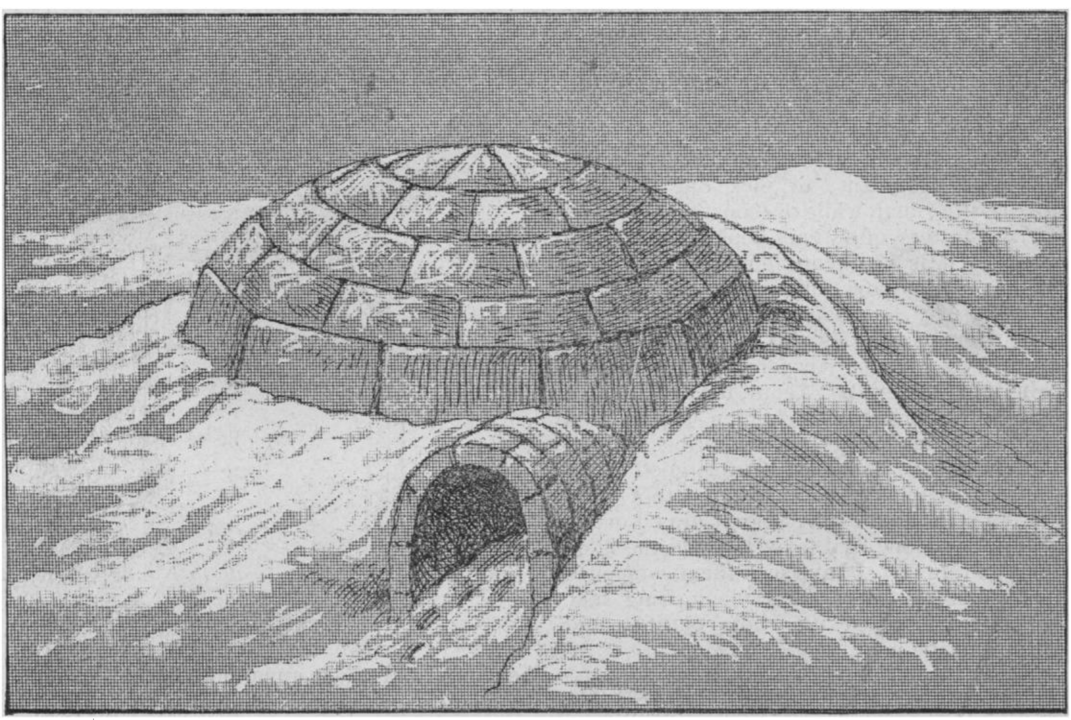
have a fair miniature representation of an ice-village.

Our canvas tent becoming very uncomfortable on account of the intense cold, which had sunk to nearly — 30° F., we had a large ice-igloo constructed, into which we moved on the 1st of November, 1878, and found it decidedly more habitable.

If the village be small, they generally construct an ice-house per day, all working, either cutting out the slabs, hauling them to the igloo site, putting them into shape, or chinking the cracks with wet snow; and this is continued until all are housed. If a large village, they divide into parties.

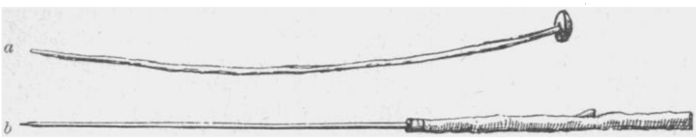
Sometimes the Innuits will retain their ice-igloo, even after the snow has become fit for building-purposes, the seal-skin tent being removed, and a new dome-shaped roof made of snow-blocks. Such cases, however, are extremely rare; and unless this combination igloo is covered in thoroughly with deep snow-drifts, or with snow thrown upon it to a depth of at least four to six feet, it will not compare in comfort with that of snow alone. The relative conductivity of the two materials, snow and ice, readily explains the reason. The ice also condenses the moisture of the breath, and the steam from cooking, more readily upon its cold, smooth surface; and this becomes at last an almost unbearable annoyance, — an annoyance which can be comprehended without explanation. The advantage of this igloo of ice is in its straight upright walls, which give more room than the slanting sides of the snow-house, while it is also easier to build, the ice portion being already constructed. We lived in such an igloo during the winter of 1878–79; but none of the Innuits around us retained theirs, and often complained of the cold when in ours, and referred it to its peculiar construction. I might add, however, that our three bedrooms or bed-igloos, which were attached to and communicated with the main one of ice, were wholly of snow.

As the reader must have already surmised from the hints given from time to time, the true igloo is built of snow, those already described being used but a very small portion of the year. It is used on all their winter journeys, even for a single night; and, as contrary to the prevailing belief, the Innuits travel the most during this season, one can see that a person sharing their life and travels would have many opportunities, during two long winters with them, to see igloo-building and igloo-life in nearly all its aspects.



AN ICE-IGLOO WITH SNOW CAPPING.

When the native has decided to relinquish his house of ice for one of snow, or on a sledge-journey has decided to go into camp, — in short, is going to build an igloo, — the first thing done is to get out the 'snow-testers,' with which they determine the compactness, depth, and general availability for building-purposes of the snow-drifts. The ancient style of snow-tester, *a*, and



SNOW-TESTERS, ANCIENT AND MODERN.

those yet used by the Esquimaux who have no trading communications with the whalers and explorers, is one made from reindeer-horn, about the diameter of a little finger, and probably three feet long. One end is sharpened, and the other, formed as a button about the size of a quarter of a dollar, is held in the palm of the hand. The modern tester, *b*, is simply the iron rod of the seal-spear with the barb removed.

Having halted on some lake that they know by certain signs has not yet frozen to the bottom,[1] the men scatter out like skirmishers along the deep snow-drifts near the shore, and commence prodding with their testers. Finally a shout from one shows that he has been successful; and, leaving the tester sticking in the snow to mark the spot, he and the others return to the sledges, which are then brought up, and the building commences.

It takes considerable experience, coupled with good judgment, to pick out the best building-site; and, while the constant prodding with the testers oftentimes looks foolish to a spectator, it is no inconsiderable part of the performance. Snow which looks perfect on the crust may be friable beyond use a few inches deeper, and this the tester will reveal. Soft drifting snow may cover a bank of splendid building-material. Again, the drift may be freely interspersed with hidden stones and bowlders, which the testers will bring to light if freely used. This testing for good snow generally occupies from ten minutes to a quarter of an hour: but I have seen it drawn out to an hour, or so long as it takes to build the igloo itself; and, in fact, I have seen them compelled to abandon the most favorable looking lake after having skirted its whole outline, and move on to the next.

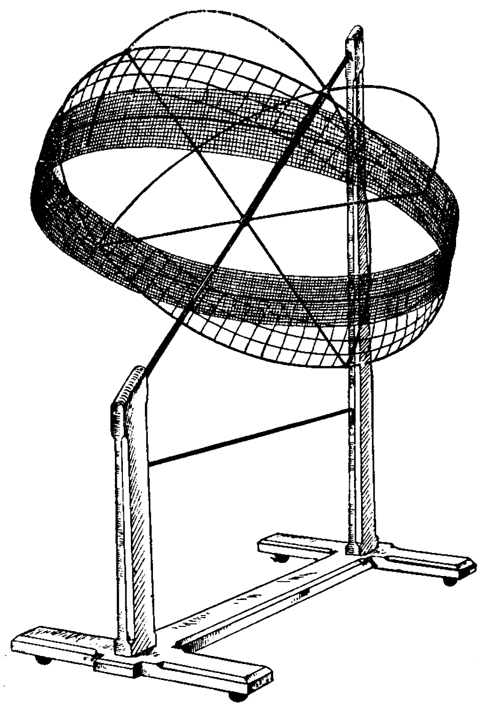
[1] This is generally done by lying flat on the ice, and placing their eyes as close to it as the nose will allow, when some varying peculiarities of the ice-colors decide their conjectures.

(*To be continued.*)

ILLUSTRATIVE APPARATUS FOR ASTRONOMY.

THE accompanying figure represents an apparatus designed for use in teaching astronomy. It is mounted so that the axis on which it rotates is parallel to the earth's axis. Two circles represent the equinoctial and ecliptic, and on the latter is a strip of wire cloth to represent the zodiac. The circles are of such a size that the meshes of the cloth (in this case a half-inch) are one degree in size, and larger meshes of five degrees are made, extending to the circle of the equinoctial. The northern halves of the two colures help to hold all in position. The lower part of these latter circles are dispensed with, so that one may conveniently stand near the centre, the frame being of such a height as to bring the centre nearly on a level with the eye.

It helps the beginner to obtain a clear conception of the fundamental circles so often referred to, of their actual position in the



heavens, and their apparent diurnal change of motion. It enables him also to represent the sun, moon, and planets in their correct

positions at any time, their right ascensions and declinations (or longitudes and latitudes) being given. For this purpose I use disks of cardboard, with small hooks attached by which they may be readily fastened to the wires. It is, besides, very convenient to use in the explanation of many questions and topics that arise in the course of the subject. A light rod or wire attached to a standard serves as a horizon when required.

The apparatus grew out of the need felt of something besides the celestial globe and the usual means of illustration for use in the lecture-room. The idea of it was suggested by a description of something like it which some one had seen; but the description was so vague, I am unable to say how nearly similar is this design, or whether it is any improvement or not on what may be used elsewhere. But I have found it to serve a very good purpose in the lecture-room, and think it may be serviceable to other teachers. G. B. Merriman.

HELL'S OBSERVATIONS OF THE TRANSIT OF VENUS IN 1769.

Professor Newcomb has lately taken advantage of a visit to the Imperial observatory of Vienna to make, with the consent and support of its director, Prof. E. Weiss, an examination of Father Hell's manuscript record, with reference to deciding on the alleged falsification of these observations by Hell himself. The result of his examination was so different from that generally accepted, that Professor Newcomb prepared and presented to the Royal astronomical society a statement of the evidence and his conclusions. The story of Hell's supposed tampering with his observations of the transit, made at Wardhus in 1769, is, in substance, that he delayed publishing them so long as to give rise to the suspicion of intending to alter them; that he showed them to no one until after he had received the observations made at other stations; that a cloud was thus thrown over their genuineness; that the suspicions thus excited were confirmed in 1835 through the discovery and publication by Littrow of Hell's original manuscript journal, which its author had neglected to destroy; and that the examination of this journal showed numerous cases of alteration and erasure of the original observed figures, including the seconds of first interior contact, which had been completely erased, and replaced by new numbers inserted with different ink at some subsequent time. And the reason for all this was supposed to be, that Hell desired to publish, not his true observations, but results which should be in the best possible accordance with the observations of others. More precise statements on some points are these: the transit occurred 1769, June 3; Hell's party sailed from Wardhus, June 27, but meeting with delays from adverse weather, and stopping to make observations, they did not reach Drontheim until Aug. 30; after some stay here and in Christiania, Copenhagen was reached on Sept. 17; the observations were communicated to the Danish academy of sciences in November or December; the printing commenced Dec. 13, and on Jan. 13, 1770, Hell received twenty printed copies. Professor Newcomb remarks that he does not know the original authority for the statement that Hell was loudly called upon for his observations before he would consent to their publication.

The document which Professor Newcomb has scrutinized is a thin manuscript volume in folio, containing twenty-seven finely written pages, and nearly as many blank ones, bearing the heading "Observationes Astronomicae et Caetera in Itinere litterario Viennâ Wardöehusium factae. 1768. A. M. Hell." This volume is assumed to be in Hell's own writing, and to be his original journal of his observations. Littrow apparently treats of it as the actual first record of the observations, but to Professor Newcomb this seems very improbable. He concludes that the writing of this journal was done at the observing-station, probably at the close of each day's work or each set of observations. What Hell sent to press in December, 1769, was not a transcript of this journal, but a more copious account, containing eighty-one printed pages, with only an occasional identity of language. But, with a single unimportant exception, the numbers are all printed without change from the original manuscript journal, whether corrected or uncorrected in that journal. It is very clear to Professor Newcomb that nearly all the alterations were made at the station — two, at least, before the ink got dry. And he further concludes, that, *whatever the sources from which the corrections were derived, the numbers as printed by Hell were all but one or two obtained at Wardhus.* Going into these manuscript corrections more in detail, it seems quite clear to Professor Newcomb that the alterations in the numbers representing the observations of first contact were made with the same ink as the original; and he regards only one conclusion as certain, — that the corrections were made at the time of writing, and without the slightest intention of giving any thing but the actually observed moment when Venus was first seen.

Coming now to the much disputed observations of internal contact, the figures of seconds seem at first sight to be corrected. Littrow says that the paper bears marks of having been scraped, and that the original figures of seconds had been carefully erased, the ink, in consequence, spreading in the paper. Professor Newcomb remarks, that one sees at a glance that the latter statement is erroneous; and he applies to the question of erasure the test of viewing the paper by oblique sunlight, and proves the texture of the surface to be still uninjured. The evidence thus leads to the certain conclusion, that no different figures from those now visible were ever written there. If, then, they are in any way the result of calculation from other observations, the place must have been left blank until Hell got back to Copen-

hagen, and made the necessary calculations, — an hypothesis too fanciful for serious discussion. Another part of the record looks more suspicious, — a line, 'fulmen 9 32 48,' is not only an interlineation, but is written in decidedly different ink from all the original manuscript. The original journal, up to the time that Hell left Wardhus, being all written in one kind of ink, we conclude that the insertion was made after he reached Copenhagen, and after he had seen the observations of others. Two hypotheses are before us as to how the insertion was determined, — we may suppose that Hell, when he found he had omitted what other observers considered an important phase, tried to remember how long after the recorded contact he first saw the sun's limb continuous, and wrote the result in his journal; or we may suppose that he made a memorandum at the time of the observation, but omitted to copy it in the journal, either through inadvertence, or because he deemed it too late for contact. When he found the phase important, he merely copied the omitted record in his journal. The use of the queer word 'fulmen,' which appears only in the manuscript, seems to Professor Newcomb to give color to the latter hypothesis. He can hardly conceive of one using it deliberately, after six months, to express the formation of the thread of light; whereas, at the moment of observation, in the excitement and hurry, it would be a very natural single word to designate the rapid increase of the effulgence of solar light around the following limb of Venus, which follows true contact at ingress. It is a strong confirmation of this view, that Mr. Stone, without apparently having made any comparison with Hell's printed observations, reached this same conclusion as to the probable use of the word 'fulmen.'

With regard to the egress of the planet, the times of Hell's notes of the 'gutta nigra' are each increased by two seconds; but obviously this correction was made at the time of writing. More serious is a correction of the time of observation by Sajnovics, the companion and assistant of Hell. They, no doubt, discussed their times; and, in consequence of such discussion, Sajnovics concluded that his times were late. In the exterior contacts, the only corrections are such as were made at the time of writing, and to which Professor Newcomb attaches no importance.

Regarding certain collateral circumstances which have been supposed to cast suspicion upon Hell's intentions, not only does Professor Newcomb see no suspicious delay in making known his observations (for the whole paper, containing an account of his instruments, observations, and results, including an investigation of his quadrant and clocks, a discussion of his latitude, longitude, and time, and a full statement of his observations, was written, printed, and ready for distribution, four months after his return to Copenhagen), but it seems difficult for him to suppose that Hell could have had time to make so complete a reduction of the observations of others as to be able to compare them with his own. That his observed times of the contacts were not published in advance, as were those of many other observers, but appeared first in an official form under the imprint of the Academy of sciences, seems to Professor Newcomb in accord with very proper feeling, as the observations were made under the auspices of the king of Denmark, and dedicated to him; and furthermore, owing to the position of the station being unknown, publication in advance could have served no useful purpose.

In his discussion, Professor Newcomb makes but slight allusion to the absence of many circumstances which might be expected to accompany manufactured observations; but he has presented all the positive evidence within reach so fully as to enable every one to draw his own independent conclusions. His own conclusions are, —

First, The belief that there was any suspicious delay in the publication of Hell's observations, or any thing in his course to give reasonable ground for a suspicion that he intended to tamper with his observations, is a pure myth.

Second, Excepting the time of formation of the thread of light at ingress; excepting, also, a discrepancy of one second in the time of internal contact, and a change of two seconds in one of Sajnovics's times, — it is proved, not only negatively and presumptively, but by positive evidence and beyond serious doubt, that all the essential numbers of observation given by Hell, whether relating to the transit, time, or longitude, are printed as concluded upon and written in his journal at Wardhus, before there was any possibility of communication with other observers.

Third, The addition of the time of the formation of the thread of light was suggested by the accounts of other observers; but the time itself is Hell's own, obtained possibly from estimation and memory, but more probably from a memorandum made at the time of observation, which he neglected to insert in his journal.

Fourth, The alterations in Sajnovics's time of second internal contact were probably made, because Sajnovics himself afterward concluded that his recorded time was too late; but it may be assumed, that, in reaching this conclusion, he was influenced by Hell's observations.

Professor Newcomb adds, respecting his own proceedings in investigating this subject, that, in commencing the examination of Hell's journal, he had no hope of doing more than deciding whether it was or was not safe to use Hell's numbers as actual results of observations, and no thought of doubting the commonly received view of the case. He soon became perplexed to find himself differing entirely from the conclusions of Littrow. Before the latter had found the manuscript, suspicion had rested upon Hell's truthfulness; so that when he looked into the manuscript, and saw such extensive alterations, the indictment seemed so clearly proven that Littrow's only duty was to make the facts which proved it, known to the world. He thus unconsciously assumed the tone of a public prosecutor, and saw all the circumstances from an accuser's point of view.

LETTERS TO THE EDITOR.

Errata in catalogues of stars.

The Washburn observatory possesses a nearly complete collection of such star-catalogues as have been printed since the year 1800. A list of them is given below. It will be noticed that this list does not include the very expensive British association catalogue. Oeltzen's Argelander's Northern zones is also missing from the list, as I have been unable to buy it in Europe during the past two years. Another very scarce catalogue (Weisse's Bessel's Zones from + 15° to — 15°) I have just obtained after two years' delay.

In each one of these catalogues, I have had every erratum known to me inserted in its proper place, so that the set of catalogues really represents what is known, freed from an enormous mass of misprints and real errors.

I am not able to say how many material errors have been corrected, but certainly not less than twelve thousand. I think those who use star-catalogues most will be most surprised at the amazing number of material errors which still remain in the catalogues which they employ daily.

I have called attention to these errors in order to say that I will engage to have any of the catalogues of the following list corrected completely, in all respects like my own, for any observatory, or for any astronomer who may desire it.

The catalogue should be sent by American express, prepaid, addressed to me, and accompanied by a note asking that the work be done. The book, when corrected, will be returned by express at the owner's expense.

The corrections will be made by one of my assistants, under my direction, and an account kept of the number of hours spent on the work. The work will be charged for at the rate of fifty cents per hour. I may say that the sum so received will be paid to the copyist.

It has appeared to me, that, after the large amount of labor which has been expended on my own catalogues, I was under obligations to give the benefit of such work to others, and this I willingly do.

The first list following gives the names of the catalogues owned by the Washburn observatory; the second gives the sources from which the errata have been derived. I shall be much indebted for references to errata not there mentioned.

EDWARD S. HOLDEN.

Washburn Observatory, Madison, Wis.,
Aug. 1, 1883.

I. List of star-catalogues.

Airy: Catalogue of 1,439 stars. 1840.0.
—— " " 1,576 " 1850.0 (6-year).
—— " " 2,022 " 1860.0 (7 ").
—— " " 2,760 " 1864.0 (7 ").
—— " " 2,263 " 1872.0 (9 ").
Argelander: Bonn observations, vols. 1–7. [Vols. 1 and 2 not corrected.]
—— Uranometria nova. [The maps not corrected.]
Baily's Lalande.
Behrmann: Uranometry. [The maps not corrected.]
Brisbane: Paramatta catalogue.
Carrington: 3,735 circumpolar stars.
Gould: Uranometria argentina.
—— D'Agelet's Catalogue.
Heis: Atlas coelestis. [The maps not corrected.]
Johnson: First Radcliffe catalogue.
Lamont: Catalogues, 6 vols.
Main: Second Radcliffe catalogue.
Robinson: Armagh catalogue.
Rümker: 12,000 stars.
Schjellerup: 10,000 stars.
Stone: Cape catalogue. 1873.
—— " " 1878.
—— " " 1880.
Weisse's Bessel's Zones I. and II.
White: Melbourne general catalogue. 1870.

II. List of sources where errata are found.

⁂ The name of the author of the catalogue comes first, then a brief reference to the particular catalogue, and, last, a reference to the place where the errata are given. No reference is made here to corrections which are usually bound in the same covers with the original catalogue, or which are given in subsequent volumes of the same work. I have also included here a few reviews which contain no errata, properly speaking.

Airy: New 7-year catalogue. — V. J. S. 1871, 100.
Argelander: Bonn obs., vi. — V. J. S. 1867, 272.
—— Durchmusterung. [A single correction to D. M.] — Astr. nachr., lxxi. col. 240.
—— - [Places of two stars not in D. M.] — Astr. nachr., lxxii. col. 55.
—— - Astr. nachr., no. 1765.
—— - Astr. nachr., no. 2396, col. 305; no. 2429, col. 69.
—— - [Ueber einen in der D. M. fehlenden stern.] — Astr. nachr., no. 2459.
—— - Astr. nachr., no. 2478. Same, no. 2527.
—— Uranometria nova. — Astr. nachr., xxvi. col. 318.
—— - Annals Harv. coll. obs., ix.
—— Northern zones. — Bonn obs., v.
—— - [Fortsetzung von band v.] — Bonn obs., vi.
—— - [Theoretical; with a few corrections to the zones.] — V. J. S. vol. 8, 221.
—— Southern zones. — Bonn obs., vi.; Cooper's Ecliptic stars, iv.
Baily's Lacaille: Catalogue. — Bonn obs., vii. 245; Cape catalogue, 1880.
Baily's Lalande: Catalogue. — Bonn obs., vii. 213 seq.; Schjellerup, 10,000 stars, p. 225.
Behrmann: Atlas des südlichen gestirnten Himmels. — V. J. S. 1875, 89.
Bessel: Zones. — Bonn obs., iv. p. i.; v. p. xxxii.
British association catalogue. — Cape catalogue, 1840; same, 1880.
Brisbane: 7,385 stars. — Cape catalogue, 1840; same, 1880.
Cape catalogue: 1840. — Stone, Cape catalogue, 1880, 559. [A single correction.]
Catalogues: Errata in standard catalogues of stars. — Monthly not. R. A. S., viii. 161. [This volume I have not access to at present.]
Copeland & Börgen: Mittlere oerter sterne zwischen 0° und —1°. — V. J. S. 1870, 197.
D'Arrest: Siderum neb. obs. hav. — V. J. S. 1868, 94.
Dreyer: Supplement to Herschel's General catalogue. — V. J. S. 1878, 274.
Ellery: First Melbourne catalogue. — V. J. S. 1876, 178; Monthly not. R. A. S., xlii. 308.
Fedorenko: Catalogue. — Bonn obs., vi.
Gilliss: Catalogue U. S. naval astron. expedition. — V. J. S. 1872, 46.
Gould: Reduction of D'Agelet. — V. J. S. 1867, 2.
—— Standard places of fund. stars. — V. J. S. 1867, 22.
—— Uranometria argentina. — Cordoba observations, ii. 295.

Groombridge: Catalogue. — In First Radcliffe catalogue.
—— - Bonn obs., vi.
Heis: Atlas coelestis. — Annals Harv. coll. obs., ix.
—— - V. J. S. viii. 67, 278; ix. 236; xiii. 111.
Herschel: Gen. cat. nebulae and clusters. — V. J. S. 1866, 176.
—— Catalogue of 10,300 double stars. — V. J. S. 1876, 61.
Johnson: First Radcliffe catalogue. — Bonn obs., vi.
Lacaille: Coelum australe stelliferum. — Bonn obs., vii.
Lalande: Catalogue. — Monthly not. R. A. S., xiv. 195. [This volume I have not access to at present.]
—— Histoire céleste. — Bonn obs., vii.
—— Observations of 1789–90. — Bonn obs., vi.
—— Catalogue. — Cooper's Ecliptic stars, iv.
Lamont: Catalogues (6 vols.) — V. J. S. ix. 94.
Main, R.: 2d Radcliffe catalogue. — V. J. S. 1870, 292.
Newcomb, S.: On the R. Asc. of the eq. fund. stars. — V. J. S. 1876, 158.
—— Catalogue of 1,098 stars. — V. J. S. 1882, 259.
Piazzi: Positiones mediae, 1814. — Bonn obs., vi.
Rümker: 12,000 stars. — Bonn obs., vi.
—— - Cooper's Ecliptic stars, iv.
—— Neuer folge. — Bonn obs., vi.
—— Preliminary catalogue of southern stars. — Stone, Cape catalogue, 1880.
Santini: First two catalogues. — Bonn obs., vi.
—— Posizioni medie di 1,425 stelle. — V. J. S. 1872, 13.
Schjellerup: Al-Sufi's Uranometry. — Monthly not. R. A. S., xliii. 266.
—— 10,000 stars. — Bonn obs., vi.
Schönfeld, E.: Catalog von veränderlichen sternen. — V. J. S. 1866, 113.
—— Zweiter catalog von veränderlichen sternen. — V. J. S. 1875, 73.
Stone, E. J.: Results of astronomical observations at Cape of Good Hope, 1856–58. — V. J. S. 1875, 192.
—— Cape catalogue, 1880. — V. J. S. 1880, 297.
Strasser: Mittlere oerter von fixsternen. — V. J. S. 1878, 88.
Struve (W.): Positiones mediae. — Bonn obs., vi.
—— - Schjellerup's 10,000 stars, p. 225.
Taylor: Madras catalogue. — Bonn obs., vi.; Cape catalogue, 1840; same, 1880.
—— Astron. obs. at Madras, 1843–47. — V. J. S. 1873, 180.
Vogel, H. C.: Positionsbestimmungen von nebelflecken, etc. — V. J. S. 1876, 276.
Weisse's Bessel's Zones, +15° to —15°. — Cooper's Ecliptic stars, iv.
—— - Gould's Astronomical journal, iii. 115. [This contains all the errata of the Astr. nachr. up to 1853, June.]
—— - Annals Harv. coll. observatory, i., pt. ii., p. lviii.
—— - Schjellerup's 10,000 stars, p. 225.
—— - Weisse's Bessel's Zones, + 15° to + 45°, p. xlv.
—— "Catalogue." — Monthly not. R. A. S., xiv. 195. [This volume I have not access to at present.]
Wilson & Seabroke: Catalogue of measures of double stars. — V. J. S. 1877, 108.
Yarnall: Catalogue U. S. naval obs. — V. J. S. 1880, 20.

The search for Crevaux.

Apropos of your recent weekly summary of the progress of geography under the titles of the Death of Crevaux, etc., I may say that a member of the French geographical society, M. Thouars, accompanied the U. S. solar eclipse expedition from Panama to Callao, March 12–21, of this year. M. Thouars had familiarized himself with explorations in South America by extensive travels in Columbia and elsewhere, and intended to penetrate the Pilcomayo region, in search of the relics of the Crevaux expedition, alone, or with only one companion, the two disguised as Catholic priests. The attempt seems foolhardy; and, for my part, I am glad to know that M. Thouars intends to carry a revolver under his priest's robe, and that he is a brave man and an excellent shot.

If he has not abandoned his daring project, we should hear of him during the early part of 1884.

EDWARD S. HOLDEN.

Madison, Aug. 6, 1883.

Occurrence of the swallow-tailed hawk in New Jersey.

Early in the evening of July 28 I was standing on the brow of the bluff overlooking the Delaware River, near Bordentown, N.J., when my attention was called to a large bird sailing in comparatively small circles high overhead. Fortunately there was a dark blue-black cloud behind it, so that I had an excellent opportunity to observe the bird. It was the swallow-tailed hawk (Nauclerus forficatus). It remained in nearly the same position for over an hour, when it altered its flight, and, with steady wing-strokes, flew rapidly in a north-west direction.

The appearance of this hawk here is one of the rarest events in the experience of New Jersey ornithologists. CHAS. C. ABBOTT, M.D.

A reckless flier.

ONE might think a tragic end would await such birds as the Swifts, so bold and persistent their flight; and doubtless such is in store for many, though they seem to steer clear of most obstacles.

A case in point came recently to hand, — that of an unfortunate bird impaled to the spear-point of a lightning-rod above a chimney. There it remained until shot off with a gun, — a warning and a ghastly one, indeed, to all this *swift* race. F. H. HERRICK.

Swallows in Boston.

I saw on the 4th of this month the first swallow in Boston, at the extreme end of City Point, South Boston. I have been on the lookout for them since April. Two friends, good observers, report that they have not seen one this season.

CARL REDDOTS.

Boston, Aug. 7, 1883.

'HAS any one seen a swallow this summer in Boston?' inquires a correspondent in SCIENCE, Aug. 3. Yes: I saw six last week, perched on the state-house. Prior to this I had also raised the query. Whether it was the pugnacious sparrows, or legislature, that had banished these aerial visitors from the capitol, their old haunt, was and is a query.

LEANDER WETHERELL.

Boston, Aug. 11.

WARD'S DYNAMIC SOCIOLOGY.

IV.

IT is Mr. Ward's theory, that the more complex sciences should be based upon the less complex. This he avowedly derives from

Comte, but himself defends at length; and his work is constructed consistently therewith. The part which relates to sociology, therefore, is based upon principles derived from the physical and biologic sciences and psychology, which he treats as a biologic science. Some general mention is made of languages, arts, and opinions, in various portions of the book; but no systematic treatment of these subjects is presented. The same is true with respect to all that body of facts which, if systematized, the author would call static sociology. He only attempts to treat, at length and in order, the forces of society. This theory is but a half-truth, and the method of treatment resulting therefrom has sometimes led to conclusions that are erroneous. The most important failure in this respect is Mr. Ward's presentation of what he denominates the four stages of society: viz., "(1) the solitary or antarchic stage; (2) the constrained aggregate or anarchic stage; (3) the national or politarchic stage; and (4) the cosmopolitan or pantarchic stage." The first or solitary stage is that which Mr. Ward supposes to exist among animals. In the second stage he supposes mankind to have multiplied in great numbers, to have been widely spread throughout the earth, and to have been aggregated without organization. The third stage is represented by the organized tribes and nations of the earth. The fourth stage is a prophecy, when all men shall be organized in one body politic.

It will be well to compare this scheme with that of Morgan in his 'Ancient society.' Morgan attempts to establish what he denominates ethical periods. The three grand periods are savagery, barbarism, and civilization; and savagery and barbarism are subdivided. The following is his scheme:—

I. Lower status of savagery . . .	From the infancy of the human race to the commencement of the next period.
II. Middle status of savagery . .	From the acquisition of a fish subsistence and a knowledge of the use of fire to the commencement of the next period.
III. Upper status of savagery . . .	From the invention of the bow and arrow to the commencement of the next period.
IV. Lower status of barbarism . .	From the invention of the art of pottery to the commencement of the next period.
V. Middle status of barbarism . .	From the domestication of animals on the eastern hemisphere, and in the western from the cultivation of maize and plants by irrigation, with the use of adobe-brick and stone, to the commencement of the next period.
VI. Upper status of barbarism . .	From the invention of the process of smelting iron ore, with the use of iron tools, to the commencement of the next period.
VII. Status of civilization	From the invention of a phonetic alphabet, with the use of writing, to the present time.

It will be seen, that Ward's scheme is consistent with his philosophy, and based on method of aggregation, while Morgan's scheme is based on the development of arts. Ward is right in his philosophic plan, but altogether wrong in its execution: Morgan is wrong in his plan, or method, but more nearly right in his final conclusions; for the three grand stages which he endeavors to establish can with some modification be fully based on the method of aggregation, i.e., on the data of sociology as distinguished from technology. This will be briefly set forth.

The inception of social organization is in the biologic differentiation of the sexes, giving husband and wife, parent and child, brother and sister, and other relations of affinity and consanguinity. At that time, when the species now known as man had made no farther progress than have some of the lower animals at the present time, this elementary organization existed; and a greater or less development of this organization is discovered among many species of the lower animals. On it the subsequent organization was built. The importance of this fundamental organization seems to have escaped Mr. Ward.

Archeologic evidence is now abundant to show, that man was widely scattered throughout the earth at a very early stage in the development of art, i.e., in the paleolithic age. Again: there is abundant linguistic evidence to show, that man was widely scattered throughout the earth at the inception or beginning of the development of articulate, i.e., organized, speech. In this condition he must have had at least something of the social organization which is based on sex. The stories which have been told, to which Mr. Ward refers without giving full credence, of men living in utterly discrete conditions, are but idle tales, and have no place in the data of scientific anthropology. Mr. Ward says, "The second stage embodies none of the elements of per-

manency, and cannot be expected to be found extensively prevailing at any age of the world. It is essentially a transition stage, and, like transition forms in biology, is characterized by an ephemeral duration. Nevertheless, it has numerous living representatives among the lower existing tribes, particularly among the Fuegians, interior Australians, Wood-Veddas, and Bushmen." The illustrations given of this second stage are also idle tales. These people must also have had the organization mentioned above as based on sex; and it is now known that some of them at least, especially the Australians, have a highly organized system of social aggregation based on kinship. These people are, in fact, organized as tribes. In the presence of facts, the first and second periods of Mr. Ward disappear.

Travellers among savage peoples, seeking for the institutions with which they were themselves acquainted among civilized men, have found them not, and have sometimes reported the peoples to be without institutions, and at other times have completely misinterpreted what they did discover. If we accept such statements, we must believe that some tribes were without organization, and some had the institutions and governments of civilization. And if we compare the statements of a number of travellers about the same people, we shall discover that most of the savage tribes of the earth have been reported, now as being destitute of government and sociologic institutions, and now as having kings, aristocracies, and the elaborate paraphernalia of civilized governments. None of these accounts are true: all are to be rejected. But there yet remains a body of sociologic data relating to the lower tribes of mankind, collected by scientific anthropologists, chiefly during the last two or three decades. We owe much of this knowledge to Morgan's researches, and the investigations of others which have grown out of his suggestions. We now know something of the organization of almost every tribe on the face of the earth, though in many cases our knowledge is exceedingly meagre and fragmentary. Yet perhaps enough is known to warrant the assertion, that there is no tribe so low but that it has a sociologic organization highly developed in comparison with that mentioned above as based on sex and exhibited among the lower animals. The outlines of this plan of organization must be set forth.

The tribes of mankind, as distinguished from nations, have each an organization based on kinship. This system of kinship invariably recognizes grades, based primarily on degrees of affinity and consanguinity, and secondarily on relative age, or the series of generations which may be extant among a people at any given time. All of the relations which exist among such a people, and which may be denominated as rights and duties, are determined by the kinship relations recognized in their social organization, and expressed in their language. This subject is too vast for thorough exposition here, and a single illustration must suffice. Among all such tribes age gives authority, but no method of determining the absolute age of any individual exists among them. Dates of birth are soon forgotten. But there is in the language of every such tribe a device by which relative age is invariably expressed; for every man, woman, and child accosts and designates every other man, woman, and child within the tribe by a term which in itself expresses relative age. Thus, in these languages there is no term for *brother;* but there is one term for *elder brother*, and another for *younger brother*. A man cannot speak of his 'brother' as such simply: he must use a term which says 'my elder brother,' or 'my younger brother,' as the case may be. In the same manner, if he speaks to or of any other person in the tribe, the term by which that person is designated will itself show the relative ages of the persons speaking and spoken to or of. Age gives authority, and this authority is so important and so universal that it is woven into the texture of every tribal language. Every tribe is organized as a great family, — a system of kindred.

From this plan of early tribal organization, there is a great development exhibited in many ways; for tribes are differentiated into classes, or clans, or gentes, which are interdependent bodies politic.

This tribal organization, so briefly characterized, has its fundamental idea in kinship; and the minds of the people in this stage can conceive of no other form of organization. If two or more tribes form an alliance, temporary or permanent, for defensive or offensive purposes, one or both, the same thought prevails. In a council for such an alliance, one of the first propositions to be settled is, 'What shall be the kinship relations existing between us?' and, before the alliance can be consummated, this must be settled.

Once upon a time the Cherokees, Choctaws, Chickasaws, Muskokees, and other tribes met in council for the purpose of forming an alliance against the upper Mississippi tribes of the Dakota stock; and it was decided, that,

as the Cherokees lived at the sources of the streams that watered the country occupied by the other tribes, they, the Cherokees, should be called 'elder brothers,' and the tribes living on the lower courses of the streams should come in order from east to west as second, third, fourth, fifth, and sixth born sons, because such was the course of the sun as it travelled over their lands. Then the people of one tribe called the people of another 'elder' or 'younger' brothers, and took precedence and authority in council and war therefrom.

This plan of organization is a distinct method of aggregation, designated as kinship, or tribal; but it gradually developed into something else. As tribes, by alliance, by conquest, and various other processes, enlarged, it was done by establishing artificial kinship, — by what Sir Henry Maine denominates a 'legal fiction;' and in many cases it came to be that the whole organization was chiefly a legal fiction. Kinship ties were chiefly artificial. Under these circumstances the kinship bond, composed of marriage-ties and streams of kindred blood, was found to be but a rope of sand; and gradually, by many steps, the basis of aggregation was changed to territory, and the bonds of society became the organs of government for the regulation of relations arising from property. But, before a territorial system of aggregation is fully established, intermediate stages are discovered. First, the tribal organization occupies a distinct territory, but the territorial organization is latent; then aggregations partly by territory and partly by kinship supervene; and finally, by many steps, kinship organization is abandoned, and territorial organization remains. This gives two very distinct methods of aggregation or plans of social organization, viz., kinship and territorial society, or tribal and national government; and the two are objectively discovered, and not simply theoretical. The first in its simplest state is Morgan's Status of savagery; the second in its simplest state is Morgan's Status of civilization. His Status of barbarism includes the higher forms of kinship organization and the transition forms mentioned above. If we confine his Status of barbarism to the transition forms, we will then have savagery, barbarism, and civilization established properly on modes of aggregation; but barbarism will merely be a transition stage, and comparatively ephemeral.

Of Mr. Ward's fourth stage, it is simply necessary to say that he himself recognizes it as an ideal of the future; but it is properly based upon history, and is in the manifest course of social evolution. Of the myriads of languages once existing, and of many of which we now have but mere glimpses, few remain, and of these few a very small number are rapidly predominating. The many have become few, and the few will be completely unified, for such is the course of philologic evolution. Of the myriads of tribes scattered by the shores of the seas, on the margins of the lakes, and along the streams of all the habitable earth, but few remain. They have been gradually integrated into larger tribes, and finally, with the most advanced, into nations; and the time will come when there will be but one body politic, for such is the course of sociologic evolution. Every tribe of the myriads that have spoken distinct languages has each for itself developed a mythologic philosophy. These mythologic philosophies are rapidly disappearing, and now are comparatively but few; and the time will come when but one philosophy will remain, — the philosophy of science, the truth, — for such is the course of philosophic evolution. The fourth stage of society — the cosmopolitan or pantarchic — is a legitimate induction, a qualitative but not a quantitative prophecy, for who shall say when it shall come?

Morgan's method of basing his stages upon the arts is unphilosophic: it was simply stages of art development, not stages of social organization. But, because art and society have evolved interdependently together, it very nearly represents the truth; but the actual condition of the progress of any given society or body politic can be determined with less accuracy from its arts than from any other department of anthropology, and this from the fact that art is expressed in material form that can be easily imitated. Its use is at once apparent; and a people may easily borrow an art, or an aggregate of arts, without passing through the stage necessary for its invention. Arts, therefore, travel beyond the boundaries of tribes, languages, and philosophies, and are rapidly spread throughout the world. Tribes that to-day use the bow and arrow may to-morrow use the gun, though they have no knowledge of chemistry and metallurgy. The attempts of the archeologists of modern times to trace migrations, or to connect peoples by a genetic tie, have been to a large extent rendered vicious by the failure to recognize this principle. Tribes and nations, peoples, bodies politic, cannot be classified by arts: but the evolution of arts may be marked off in stages, as done by Morgan; and his stages are the

best yet proposed, though he failed as an ethnologist in the attempt to classify races.

In the same manner, but to a less degree, scholars have failed to classify peoples by languages; for languages only to a limited extent represent genetic connections of peoples. Tribes speaking diverse languages have coalesced; and languages have thus been compounded, and language has supplanted language. A linguistic classification, therefore, is not completely ethnic, but it comes nearer to the truth than the technologic classification. If a classification by philosophies were attempted, it also would fail, though it would be superior to the philologic; for opinions last longer than words. A sociologic classification of peoples also fails to exhibit genetic relationships. Arts, languages, states, philosophies, may be classified, each to show genetic relationships; but they each and all together fail to classify mankind in a fundamental and philosophic manner.

Scholars have devoted much time and ingenuity to classify mankind by biologic characteristics, sought for in the color of the skin, the texture of the hair, the form of the skull, the relative proportion of parts, etc. These attempts have all failed. It is probable that in the early history of mankind biologic differentiation progressed so far as to produce some well-marked varieties; but the biologic method of evolution by the survival of the fittest was more and more repealed as the anthropologic methods of evolution gained ground, and the scattered and discrete tribes were more and more commingled by the union here and there of distinct streams of blood, by the spread of arts, that placed all peoples under conditions of artificial environment, and made them more and more independent of natural environment, and by various other anthropologic conditions too numerous and complex to be here set forth. But, altogether, the tendency to differentiate into distinct biologic peoples has been overcome, and the tendency to unification has been steadily increasing: so that the distinctions of biologic varieties of mankind, of which we now have but hints in the biologic characteristics remaining, are gradually being obliterated; and we may confidently predict that in the fourth stage, yet to be reached, race distinctions will be utterly lost.

In the short articles of this review an attempt has been made to give a synopsis of the work in question, to show the relation of 'Dynamic sociology' to current philosophy, and to point out its more important defects. Little space is left for that commendation which its intrinsic merits deserve. Mr. Ward's presentation of the subject is simple, clear, systematic, and courageous. For its preparation he has explored vast fields of thought; and his conclusions, however they may be questioned, cannot be ignored by those who are interested in modern philosophy. Ward's Dynamic sociology is America's greatest contribution to scientific philosophy.

ELEMENTARY METEOROLOGY.

Elementary meteorology, with meteorological charts and illustrations. By R. H. SCOTT. London, *Kegan Paul, Trench, & Co.*, 1883. 408 p. 8°.

THIS volume, the latest English contribution to the science of meteorology, is not a treatise, as the title indicates. It is, however, an excellent work, treating the subject from a modern stand-point, and sweeping away many untenable theories. We especially note the chapters on the barometer and on the formation of rain and hail. The descriptive chapters collecting all known facts relating to wind and ocean currents are very valuable and well presented.

Our author rejects the once seemingly satisfactory theory, attributing the south-west monsoon winds of India to the rising of heated air above the plains to the north-east of the Himalaya range, and also the theory that the existence of sea-breezes is due to the rising of heated air upon the land near oceans. He, however, adopts this theory of ascending currents of heated air in explaining the formation of cumulus-clouds. It is difficult to see how the atmosphere can be heated, save gradually, in strata parallel to the earth's surface, except on mountain sides. This is the theory adopted by Hann, who regards the cumulus-cloud as simply indicating the layer at which the air has the temperature of the dew-point.

Mr. Scott seems to indorse the theory that there is an ascending current in the centre of a barometric depression, though his storm-chart on p. 355 shows all the wind-directions near the low centre tangent to the isobars. This shows that the air-motion, which at the outside of the storm is directed more or less toward the centre, gradually becomes circular as it approaches the centre. Such a whirl moving over the earth's surface, losing a part of the air in its path, does not require any ascending current at its centre. The same may be said of our author's theory that rain can be formed by rising currents of heated air. In this case, not only is there the doubtful assumption of an ascending current, but

the formation of rain under these circumstances seems disproved, in another place, by the author himself, who rejects the theory that any considerable precipitation can be produced by the mixture of masses of hot and cold air. Mr. Scott acknowledges that nothing definite is known as to the origin of atmospheric electricity; but his conjecture that the coalescence of cloud-droplets into rain-drops may be due to electricity will hardly be accepted by meteorologists at present. The description of a peculiar electrical manifestation observed in the Alps, July 10, 1863, is very similar to that given by Siemens while on Cheops pyramid, April 14, 1859.

The division of thunder-storms into heat and cyclonic is hardly applicable to the United States, where it appears as if no thunder-storms occur, except as largely influenced by, or directly dependent on, the presence of a barometric depression.

The error of more than forty million square miles in the earth's surface between the equator and 30° north latitude should be corrected in the next edition.

The statement, that at great depths in the ocean a probable uniform temperature of 32° F. prevails, has been disproved by the researches of Professor Verrill and the U. S. fish-commission.

We notice on p. 362 the surprising statement, that, as the central office of the U. S. weather bureau is in the eastern part of the country, there is a great advantage to those predicting storms by the use of the telegraph.

The chart of mean January isobars does not incorporate Stelling's work in Siberia, published in 1879, and accepted by Mohn in the last edition of his Meteorology. Mohn's chart shows a mean pressure over central Siberia of 780 mm. (30.79 in.), while the highest figure in Scott for the same region is 30.4 inches.

AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

The thirty-second annual meeting of the American association was opened in the halls of the university of Minnesota, Minneapolis, Aug. 15, at 10.30 A.M. Dr. J. W. Dawson, the retiring president, introduced the president elect, Prof. C. A. Young, who briefly and gracefully expressed his thanks to the association for the distinction they had offered him. After welcomes spoken by the governor of the state and the mayor of the city, the principal address was made by the acting president of the university, Dr. W. W. Folwell, on behalf of the local committee. From his address we print the closing sentences: —

I should do a wrong to my city if I should leave upon you the impression that we are so overwhelmed and engrossed with our material labors as to have no care for the things of the mind and the higher life. If that were true, why should we welcome with so much sincere ardor the assemblage of your association? From the villages of New England, and the farmhouses of the Middle states, our people have brought that perennial curiosity, that thirst for knowledge, that intense though sombre imagination, which have given American civilization and American literature a cast and hue of its own. I must, in a word, praise our system of public schools, both city and state, which under able management and popular support cannot, we believe, be ranked below those of any communities of our size in the Union. Minnesota is the first place which has organized its secondary as well as its primary education, and offered to every child in the state a free course of studies, from the alphabet to the degree of master of arts. Our churches, goodly in size and number, may speak for the interests of religion. The future will attest the diligence and the fidelity of those who love music and the sister arts, of whom far older cities might be proud. It is thus, however, Mr. President, that we Minneapolitans, alert, pre-occupied, pause in the midst of our labors to welcome your already venerable association. We hail you as the survivors of a generation of great investigators, — the Sillimans, the Baches, the Morses, the Rogerses, who have made their own country famous and their own names as imperishable as science herself. We hail you as the worthy successors of such a generation, perpetuating and enlarging their work. In common with civilized people, we recognize the immense debt of the modern world to science; yet often, no doubt, while we are filling the sky with applause to some lucky inventor, we are not remembering the years, perhaps generations, of inconspicuous and painful labors, carried on in our studies and laboratories, which made the invention possible. Let the inventor have his glory and his profit without envy and without stint; but let us not fail to build the cenotaph of a thousand nameless geometers, star-gazers, and natural philosophers, who, working in silence and obscurity, without thought of fame or hope of reward, put it in his power to bless and captivate the world. We are grateful, therefore, to science for the telegraph and the microscope, for chloroform, for the photograph, for all the nameless applications of electricity. To science we owe that magnificent apparatus of transportation which is the crowning and distinctive feature of modern material life. To

science we owe the thousand appliances which yield comfort and even elegance to the humblest household. Immense as are these contributions of science to material comfort and happiness, she has still, I think, performed greater services to mankind. The scientific method developed in the study of nature has spread to all branches of investigation. It has permeated all our education: it has boldly leaped the boundary between physics and metaphysics. It has even penetrated into industry and business and common life. The modern man first collects what knowledge he can about his enterprise or adventure, and assures himself of its value. He then makes the best quest he can in regard to the future. Then he assembles new facts, and, as the facts require, revises and amends his theory, till at length it becomes a working rule, maxim, and principle. He knows not merely how to know, but how to guess. The penetration of the scientific method into the operations of trade in great commercial centres is very conspicuous. We even endeavor to gamble scientifically. No Drew, or Armour, or Gould ever forms his corner without a most careful study of the situation; and his venture is his bet on the correctness of his theory. The farther extension of the scientific method, till it shall become the guide of conduct in the every-day life of all men, is now the chief problem in education.

In the next place, I think science may at length fairly claim to have wrought out, under great difficulties, a working hypothesis of our universe in the nebular hypothesis and its almost necessary corollary, 'evolution.' It cannot be denied that we are all, in some sense, evolutionists, — some of us against our prepossessions, some of us by insensible but progressive lapses. I am not competent to argue out this great theme. I feel bound to admit that the evolution doctrine, in one form or other, has quietly taken possession of the modern mind. Why may we not gladly accept it as a most useful working hypothesis of the mode of creation? I say, of the mode of creation; for the mystery of creation will forever mock the powers of man. Only this we know: that unless human consciousness is a juggle, and human language a mockery, there can never be to man a creation without a creator, nor an evolution without an evolver.

Another great service of science is the maintenance in the world of a body of men, a lay priesthood, devoted to the search for truth for its own sake and its own value. In a mercenary age, when, in the opinion of a distinguished contemporary, mercantilism has become a huge disease and excrescence on society, the example of such a body of men is of supreme value in the training of the new generations. Youth are formed, a wise Greek has taught us, not so much by schools as by the example of distinguished men.

A still greater benefit of science to mankind is the emancipation it has wrought for us, in the last generation, from superstition and the dominion of imaginary powers. It is no long time since it was generally believed by civilized men, that human affairs were under the control of the spirits of the air, good or evil. Men walked in cringing terror, by day and night, of demons and goblins damned. The earthquake, the tornado, the lightning's stroke, they looked upon as instruments of punishment for the sins of rulers and peoples. Thanks to science, the modern world has emerged from this cloud of gloom. We have some certain knowledge. Knowledge is not merely qualitative, but quantitative. Truth ever makes free. Above all, we know that all things in nature are governed by law, — law, "whose seat is in the bosom of God, whose voice is the harmony of the world." The beautiful conception of the Greeks of the universe as a kosmos, that is, an embodiment of divine and perfect order, is pervading modern thought. We now know that the phenomena of nature have no relation to human conduct, the impartial rain falling alike on the just and unjust. Men walk the earth erect and free, fearing no bogies, or warlocks, or demons of any kind. How vast and how blessed the relief to childhood! In dispelling superstition, science has incidentally wrought her greatest service to mankind in the purification of religion. The time is coming when grateful thanks will be rendered by the minister of religion for the emancipation which science has wrought for the faith; when the conflict of science and religion will only be remembered as the antagonism of crude theories on the one hand, and cruder superstitions on the other. Grateful we are for the knowledge which science has collected and collated and perpetuated to our use. All honor to the men who are consecrated to truth in her service! We may not know what marvels, far surpassing all the gifts of the past, the science of the future may reveal. Still, we must remember that the human mind is finite, while truth is infinite. The vast unknown engirdles our little circle of light. The mystery of life and death, no son of earth has ever penetrated. Welcome, then, the faith which points to the continuance of life in a land where study will be no weariness to the soul, where no veil of flesh will cloud the vision, where science and religion shall be forever one, where men shall know even as they were known.

To welcome you as a body of scientists, lovers and seekers after truth from love of it and of your kind, would be well worth our while, were it our only motive to improve and inspire the children and youth of our city. In doing you honor, we give them a lesson no books nor masters could impart. For their sake we renew our welcome.

President Young briefly responded: —

Gentlemen, — On behalf of my fellow-members of the association, I return you my sincerest thanks for the hearty welcome we have received to this magnificent state, this young and beautiful city, this vigorous, energetic, warm-hearted community. When you first invited us here, it was not in our power to come; but your second invitation we have accepted most gladly, and hope and believe that our meeting here will prove a benefit and pleasure to all con-

cerned. Some of us have known you personally before, and most of us have long been more or less familiar at second hand with your state and city; and yet, I think, to many of us it is something like a new revelation to see for ourselves what a few years have accomplished. I am not enough of a Latin scholar to quote my Virgil well; but I have been all the time most forcibly reminded of the passage in which Æneas first comes in sight of rising Carthage. Most emphatically the work 'hails' here. We see no drones or sluggards; but every shoulder is at the wheel, and every thing is moving. It may, perhaps, seem to you sometimes, when in our sectional meetings we discuss some question about the stars, or some hypothesis as to the formation of rock-strata, or the structure of some worm or insect, that we are out of the current, and contributing nothing to the advancement of the world. But you know it is not so, and your invitation to hold our meeting here shows that you know it. The world advances, not on one line only, but on many, — on lines material, intellectual, spiritual. To some extent, the movements are indeed independent, but not very far. Any true advance on either line implies corresponding movement on each of the others, if not absolutely simultaneous, yet surely consequent. There is no need to ask you here how much this city owes to modern science, when I see on every side, in your streets and storehouses and mills, the practical application of the highest engineering, mechanical, and electric art; and in the future it is almost certain that science is to contribute still more liberally to business. But not mainly for this reason do I claim your regard to science; but because, made in the image of God as we are, knowledge and understanding are as truly wealth and power as lands and food and money.

I need not add that, as you have invited us here, so we on our part cordially invite you to attend all our meetings, to listen to the papers and their discussion. We cannot promise that every paper will be interesting to all, but each one, I think, will be able to select certain ones he will be glad to hear; and if any of you choose to join us, and enroll yourselves as promoters of the advancement of science, our membership is open on easy terms. Once more, gentlemen, we thank you for the cordial welcome, and address ourselves to our business, in the hope and confidence that our meeting here is to be in the highest degree pleasant and successful.

PROCEEDINGS OF SECTION A. — MATHEMATICS AND ASTRONOMY.

ADDRESS OF WILLIAM A. ROGERS, OF CAMBRIDGE, MASS., VICE-PRESIDENT OF THE SECTION, AUG. 15, 1883.

THE GERMAN SURVEY OF THE NORTHERN HEAVENS.

THE illustrious Argelander was accustomed to say, in the quaint form of speech which he often employed, "The attainable is often not attained if the range of inquiry is extended too far." In no undertaking is there greater need of a judicious application of this sound maxim than in the systematic determination of the exact positions of all the stars in the visible heavens which fall within the reach of telescopes of moderate power.

The first subject which engaged the attention of the Astronomische gesellschaft, at its formation in 1865, was the proposition to determine accurately the co-ordinates of all the stars in the northern heavens down to the ninth magnitude. To this association of astronomers (at first national, but since become largely international, in its character and organization) belongs the credit of arranging a scheme of observations by which, through the co-operation of astronomers in different parts of the world, it has been possible to accomplish the most important piece of astronomical work of modern times. With a feasible plan of operations, undertaken with entire unity of purpose on the part of the observers to whom the several divisions of the labor were assigned, this great work is now approaching completion. While it is yet too early to speak with confidence concerning the definitive results which the discussion of all the observations are expected to show, we may with profit consider the object sought in the undertaking, the general plan of the work, the difficulties which have been encountered, and the probable bearing which the execution of the present work will have upon the solution of a problem concerning which we now know absolutely nothing with certainty, — a problem of which what we call universal gravitation is only one element, if, indeed, it be an element, — a problem which reaches farther than all others into the mysteries of the universe, — the motion of the solar and the sidereal systems in space.

Our first inquiry will be with respect to the condition of the question of stellar positions at the time when this proposal was made by the gesellschaft in 1865. All the observations which had been made up to this time possess one of two distinct characteristics. A portion of them were made without direct reference to any assumed system of stellar co-ordinates as a base, but by far the larger part are differential in their character. This remark holds more especially with reference to right ascensions. Nearly all of the observations of the brighter stars made previous to about 1830 were referred to the origin from which stellar co-ordinates are reckoned by corresponding observations of the sun; but since that date it has been the custom to select a sufficient number of reference stars, symmetrically distributed both in right ascension and declination, and whose co-ordinates were supposed to be well known. The unequalled Pulkova observations for the epoch 1845 form, I believe, the only exception to this statement. From the assumed system of primary stars are derived the clock errors and instrumental constants which are employed

in the reduction of all the other stars observed. The positions of these secondary stars, therefore, partake of all the errors of the assumed fundamental system, in addition to the direct errors of observation.

The following list comprises the most important of the catalogues which have been independently formed; viz., Bessel's Bradley for 1755, the various catalogues of Maskelyne between 1766 and 1805, Gould's D'Agelet for 1783, Piazzi for 1800, Auwer's Cacciatore for 1805, Bessel for 1815, a few of the earlier catalogues of Pond, Brinkley for 1824, Bessel for 1825, Struve for 1825, Bessel for 1827, Struve for 1830, Argelander for 1830, and Pulkova for 1845.

The important catalogues of secondary stars published previous to 1865 are comprised in the following table.

[*Table omitted.*]

An analysis of these catalogues reveals four important facts: —

First, that a large share of the observations relate to bright stars, at least to stars brighter than the eighth magnitude.

Second, that in a large number of cases the same star is found in different catalogues, but that no rule is discoverable in the selection.

Third, that with the exception of the polar catalogues of Fedorenko, Groombridge, Schwerd, and Carrington, the double-star observations of Struve, and the zone observations of Bessel and Argelander, the observations were not arranged with reference to the accomplishment of a definite object.

Fourth, that each catalogue involves a system of errors peculiar to the observers, to the character of the instrument employed, and to the system of primary stars selected, but that thus far there had been no attempt to reduce the results obtained by different observers to a homogeneous system. In estimating the value of these observations it will be necessary to refer to the researches which have been made subsequent to 1865.

The systematic deviations of different catalogues in right ascension *inter se* were noticed at an early date by several astronomers; but the first attempt to determine the law of these variations seems to have been made by Safford in a communication to the monthly notices of the Royal astronomical society in 1861 (xxi. 245), 'On the positions of the Radcliffe catalogue.' I quote the equation derived by Safford, since it appears to be the first published account of a form of investigation almost exclusively followed since that time. It is as follows: —

Diff. of R. A. (Greenw. 12 Year cat. — Rad.) = $-0.38^s. + 0.32^s. \sin(\alpha + 5\,\text{h.}\, 32\,\text{m.})$. Extending this expression to terms of the second order, it may be put under the form, $\Delta = a$ constant $+ (m \sin \alpha + n \cos \alpha) + (m' \sin 2\,\alpha + n' \cos 2\,\alpha) +$, etc.

Safford also seems to have been the first to notice the connection between the observed residuals, and the errors in position of the primary stars employed. He remarks, "In investigating the causes which would give rise to such systematic discrepancies, I was struck with the fact that the same or nearly the same variations were apparent in the assumed places of the time stars for the years since 1845; that, if the correct positions of the time stars had been assumed, the resulting positions would have been free from these small errors." That the relation given by Safford should have been observed at all, is the more remarkable, since the primary stars upon which the Radcliffe positions depend are nearly the same as those employed at Greenwich. In reality, the systematic errors of both catalogues have since been found to be considerably greater than is here indicated, and the deviation pointed out by Safford is in the nature of a second difference. The speaker has shown (*Proc. Amer. acad.*, 1874, 182) that the weight of the errors of the provisional catalogue assumed, fell between the first and the third quadrants in the Radcliffe observations for 1841–42, on account of the omission of certain clock stars which were used at Greenwich.

Since the discordances which exist between two catalogues may arise from errors in either one or in both, it is clearly impossible either to determine the nature of the erorrs, or to assign their true cause, until a fundamental system has been established which is free both from accidental and from periodic errors, — from accidental errors, since a few abnormal differences may easily invalidate the determination of the errors which are really periodic; from periodic errors, because a relative system can only become an absolute one when one of the elements of which it is composed becomes absolute.

We owe to the researches of Newcomb, published in 1869–70, a homogeneous system of stellar co-ordinates in right ascension, which are probably as nearly absolute in their character as it is possible to obtain from the data at present available. He determined the absolute right ascensions of thirty-two stars of the first, second, and third magnitudes, and comprised between the limits $-30°$ and $+46°$ declination. A comparison of the places of these stars for a given epoch, with the same stars in any catalogue for the same epoch, enables us to determine with considerable precision the system of errors inherent in that catalogue. Several circumstances prevent the exact determination of this relation. Among them may be mentioned the fact that Newcomb's system cannot safely be extended far beyond the limits in declination of the stars composing the system, that the stars are not symmetrically distributed in declination, and that the system of errors derived from bright stars is probably not the same as that derived from stars of less magnitude.

To a certain extent all of these objections have been met in the later discussion by Auwers, to which reference will presently be made. The substantial agreement of these two systems, independently determined, furnishes satisfactory evidence that we have at last obtained a foundation system with which it is safe to make comparisons, from which we may draw conclusions with comparative safety. When the catalogues which were formed between 1825 and 1865 are compared with Newcomb's fundamental system, through the medium of these thirty-two stars, the following facts are revealed.

a. The only catalogues in which there is freedom from both accidental and periodic errors are Argelander's Åbo catalogue for 1830, and the Pulkova catalogue for 1845. One is reminded, in this connection, of the remark of Pond, that "we can hardly obtain a better test of our power of predicting the future positions of stars than by trying by the same formula how accurately we can interpolate for the past. In a variety of papers which I have submitted to the Royal society, I have endeavored to show, that, with us, the experiment *entirely* fails."

b. During this interval the constant differences between the earlier catalogues and Newcomb's system vary between $+0.17^s$ for Pond, 1820; and -0.19^s for Pond, 1830: and for later catalogues, between $+0.07^s$ for Cambridge, 1860; and $+.02^s$ for Greenwich, 1860.

c. All the right ascensions determined at English observatories, and especially those which depend upon the positions published by the British Nautical almanac, are too large in the region of five hours, and too small in the region of eighteen hours. The general tendency of the constant part of the deviation from Newcomb's system is to neutralize the periodic errors in the region of five hours, and to augment them in the region of eighteen hours, where, in the case of a few catalogues, the error becomes as great as 0.10^s, — a quantity which can be readily detected from the observations of two or three evenings with an indifferent instrument, if it relates to a single star.

The right ascensions determined at French observatories exhibit systematic errors, which follow nearly the same law as those which characterize English observations.

Distinctively German observations are nearly free from systematic errors. As far as they exist at all, their tendency is to neutralize the errors inherent in distinctively English and French observations.

d. In the case of several catalogues, residual errors of considerable magnitude remain after the systematic errors depending upon the right ascensions have been allowed for. These errors are found to be functions of the declination of the stars observed, and without doubt have some connection with the form of the pivots of the instrument with which the observations were made. This statement holds true, especially with respect to the observations at Paris, Melbourne, and Brussels, between 1858 and 1871; and to the Washington observations between 1858 and 1861.

e. The systematic errors which exist in observations previous to 1865 follow the same law, and have nearly the same magnitude, as the errors of the same class which are inherent in the national ephemerides of the country in which they were made.

The British Nautical almanac and the Connaisance des temps are largely responsible for the perpetuation of this class of errors. For a few years before and after 1860, the ephemerides of the Nautical almanac were based upon the observations of Pond, which contain large periodic errors. It is found that the errors of this system have been transferred without sensible diminution to every catalogue in which the observations depend upon Nautical almanac clock stars. At English observatories it has been the custom to correct the positions of the fundamental stars by the observations of each successive year; but this has produced no sensible effect on the diminution of the periodic errors, which belong to the fundamental system. The periodic errors of the American ephemeris follow nearly the same law as the errors of the Nautical almanac, but their magnitude is somewhat reduced. The error of equinox is also less.

Wolfer's *Tab. reg.*, upon which the Berliner jahrbuch is based, has no well-defined systematic errors; and the correction for equinox is nearly the same in amount as in the American ephemeris, but with the opposite sign. The accidental errors seem to be rather larger than in the system of the American ephemeris.

f. A general estimate may be formed of the relative magnitudes of the errors of secondary catalogues by comparing the average error for each star of the primary catalogue. The numbers given below represent the average deviation for each star, expressed in hundredths of seconds, after the various catalogues have been reduced to a common equinox.

		Average error for each star.
Argelander	1830	1.1
Pulkova	1845	1.1
Greenwich	1845	2.0
Greenwich	1860	2.0
D'Agelet (Gould)	1783	2.2
Cape of Good Hope (Henderson)	1833	2.2
Greenwich	1850	2.2
Greenwich	1871	2.2
Paris	1867	2.4
Washington	1846-52	2.5
Struve	1830	2.5
Cape of Good Hope	1856	2.8
Radcliffe	1860	3.1
Greenwich	1840	3.1
Bessel	1825	3.2
Pond	1830	3.7
Gillis	1840	3.8
Madras (Taylor)	1830	3.9
Cape of Good Hope (Fallows)	1830	3.9
Radcliffe	1845	4.5
Armagh	1840	5.0
Piazzi	1800	5.3
Bessel's Bradley	1755	7.9
Lalande	1800	13.2
Lacaille	1750	24.9

It is obvious from these relations, that previous to about 1825 the magnitude of the accidental errors of observation, combined with the errors of reduction, prevent any definite conclusions with respect to the periodic errors inherent in these early observations. It is probable, also, that early observations of stars of the eighth and ninth magnitudes are subject to a class of errors peculiar to themselves, the nature of which it is now well-nigh impossible to determine.

The systematic errors in declination which belong to the various secondary catalogues named are even more marked than those in right ascension. The experience of Pond in 1833 is the experience of every astronomer who has attempted to compare observations of the same star made at different times, under different circumstances, with different in-

struments, and by different observers. He says, "With all these precautions, we do not find, by comparing the present observations with those of Bradley made eighty years ago under the same roof, and computed by the same table of refractions, that we can obtain by interpolation any intermediate catalogue which shall agree with the observations within the probable limits of error."

We owe to the investigations of Auwers (*Astron. nachr.*, nos. 1532–1536), the first definite system of declinations which is measurably absolute in its character. Yet the deviations of this system from that derived by the same author, but from much additional data in publication xiv. of the gesellschaft, is no less than 1.2″. The present difference outstanding between the Pulkova and the Greenwich systems at 10° south declination is 1.7″.

Within the past five years, the labors of Auwers, of Safford, of Boss, and of Newcomb, have resulted in the establishment of a mean system of declinations from which accidental errors may be considered to be eliminated in the case of a large number of stars; but the different systems still differ systematically *inter se* by quantities which are considerably greater than the probable error of any single position.

When the discussion of the question of a uniform determination of all the stars in the northern heavens to the ninth magnitude was taken up by the gesellschaft at its session in Leipzig in 1865, Argelander, who was then president of the society, appears to have been the only astronomer who had a clear apprehension of the difficulties of the problem. He alone had detected the class of errors whose existence subsequent investigations have definitely established. He alone had found a well-considered plan by which these errors might be eliminated, as far as possible, from future observations.

Argelander, however, always claimed for Bessel the first definite proposal of the proposition under consideration (see *Astron. nachr.*, i. 257). It was in pursuance of this plan that the zones between − 15° and + 15° in declination were observed. These zones were to form the ground-work of the Berlin charts; and Argelander, in the execution of the Bonn Durchmusterung, simply carried out the second part of Bessel's recommendation.

With the exception of the observations of Cooper at Makree observatory, and the charts of Chacornac, these two great works — the second being a continuation of the first, under a better and more feasible plan — are the only ones in existence which give us any knowledge of the general structure of the stellar system.

The observations of stars to the ninth magnitude, found in the catalogues of Bessel, Lalande, and Piazzi, form the ground-work of these charts. The co-ordinates in right ascension and declination of the stars found in these authorities were first reduced to the epoch 1800; the resulting right ascension being given to seconds of time, and the declination to tenths of minutes of arc. With these places as points of reference, all other stars were filled in, down to the ninth magnitude, by observations with equatorial instruments. The work was divided into zones of one hour each. Bremiker undertook five zones; Argelander and Schmidt, two; Wolfers, three; and Harding, two. The remaining zones were undertaken by different astronomers in widely separated localities.

The work seems to have been performed with somewhat unequal thoroughness, some zones containing nearly all the stars to the ninth magnitude, while in others a large number of stars having this limit in magnitude are wanting.

The Durchmusterung undertaken by Argelander at Bonn was a far more serious and well-considered undertaking. This unequalled work consists in the approximate determination of the co-ordinates of 324,198 stars situated between — 2° and + 90° declination. It includes stars to the 9.5 magnitude, the co-ordinates being given to tenths of minutes of time, and the declinations to tenths of minutes of arc.

The first definite proposal of this work undertaken by the gesellschaft, however, appears to have been made by Bruhns. In the course of a report upon the operations of the Leipzig observatory, he stated, that, in his view, the time had come for undertaking a uniform system of determinations of the places of stars to the ninth magnitude in the northern hemisphere by means of meridian circles; but he proposed, at the same time, that the positions of stars fainter than the ninth magnitude should be determined by means of differential observations with equatorial instruments. After explaining certain plans and arrangements relating particularly to his own observatory, he introduced the following resolution: —

"The Astronomische gesellschaft regards it as needful that all the stars to the ninth magnitude, occurring in the Durchmusterung, should be observed with meridian circles, and commissions the council to arrange for the execution of the work."

This proposal occasioned a long and somewhat animated discussion, in which Argelander, Hirsch, Bruhns, Förster, Schönfeld, and Struve took part.

Argelander declared himself surprised at this proposal, which called for the rapid realization of a plan of organization which he had been considering for years with the greatest care, the difficulties of which he had maturely considered, and the execution of which still demanded the most careful deliberation and preparation. One of the necessary preliminary steps was a plan which he had already prepared, published and presented to the society in an informal way, which provided for contemporaneous and corresponding observations of the brighter stars. As president of the society, he felt unequal to undertaking the charge which the acceptance of the resolution proposed would involve; as this procedure seemed to him premature without previous preparation. He would admit, however, that every call to action of this kind tended to stimulate enthusiasm, and should therefore be encouraged; but he felt obliged to ask the society not to require from him the immediate execution of the plan, but to intrust the serious con-

sideration of it, and the preparation for it, to his zealous friends in the council.

Upon the motion of Struve, the society by a rising vote, expressed its confidence in the assurance of the president that he would bring forward his plan at the proper time, as soon as the means for its execution could be assured.

At the meeting held at Bonn in 1867, Argelander again brought up the subject in a communication which appears to have been an exhaustive discussion of the whole problem. This paper is not printed in the proceedings of the gesellschaft; but at its conclusion a committee was appointed to take definite action with respect to the recommendations which it contained. The committee reported at the same session; and their report, which is published in the place of the paper presented by Argelander, is probably identical in substance with it. The plan proposed and adopted was finally published in the form of a programme, in which the details of the work are arranged with considerable minuteness. As this programme has been widely distributed, it seems unnecessary to give any thing more than a general abstract of it. Since it differs in a few minor points from the first report of the committee at the Bonn meeting, the essential features of this report will be given instead of an abstract of the programme itself.

They are as follows: —

a. The limits in declination of the proposed series of observations are — 2° and + 80°. The first limit was chosen on account of the lack of suitable fundamental stars south of the equator. It is probable, also, that Argelander had a suspicion of the fact, since proven, that the uncertainty with respect to the systematic errors of southern stars is, of necessity, considerably greater than for northern stars, and that on this account it would be better to defer this part of the work until further investigations in this direction could be made.

The limit + 80° was chosen because the repetition of Carrington's observations between 81° and 90° was considered superfluous, and Hamburg had already undertaken the extension of Carrington's observations from 81° to 80°.

b. Within these limits, all stars in the Durchmusterung to the ninth magnitude, and, in addition, all stars which have been more exactly observed by Lalande, by Bessel at Koenigsberg, and by Argelander at Bonn, are to be observed.

c. The observations are to be differential. The clock errors are not to be found from the fundamental stars usually chosen for this purpose, and the equator point corrections are not to be derived from observations at upper and lower culminations, but these elements are to be derived from a series of 500 or 600 stars, distributed as uniformly as possible over the northern heavens. The exact co-ordinates of these stars are to be determined at Pulkova, thus securing the unity necessary in order to connect in one system the observations of different zones.

d. Every star is to be observed twice. If the two observations differ by a quantity greater than ought to be expected, a third observation will be necessary.

e. In order to facilitate the work, it will be desirable to use only three or four transit threads, and only one or two microscopes. In order to facilitate the reductions to apparent place, the working-list of stars should be comprised within narrow limits.

f. Before the commencement and after the close of each zone, two or three fundamental stars are to be observed upon the same threads and with the same microscopes as were used in the zone observations. When the seeing is not good, and when for any other cause it seems desirable, one or more fundamental stars may be observed in the course of the zone. The number and selection of the stars will depend upon the character of the instrument employed. If it remains steady for several hours, and has no strongly marked flexure or division errors, or if these errors have been sharply determined, the fundamental stars may be situated ten degrees or fifteen degrees away from the zone limits. However, there must remain many things for which no general rule can be given, and which must be left to the judgment of the observer, aided by an accurate knowledge of his instrument.

g. With a Repsold or a Martin instrument, one microscope will be sufficient, if its position with respect to the whole four can be determined. It will be sufficient, if the change in position during the observations can be interpolated to 0.2″.

h. It will be desirable to divide beforehand the zones into such time intervals that the observations can be easily made.

i. Zones exceeding one and one-half or at the most two hours are not advisable, first, because the zero points will be too far apart, and, second, because a longer duration will involve too much fatigue physically and mentally.

At the conclusion of this report, all the astronomers present who were willing to take part in this work were requested to communicate with the council, stating the region of the heavens which they preferred to select for observation.

At this meeting, Berlin, Bonn, Helsingfors, Leipzig, and Mannheim signified their intention to share in the work. Leiden also expressed its intention of taking part as soon as the work already undertaken should be completed.

When the stars to be observed had been selected from the Durchmusterung, it was found that the number would not vary much from 100,000, requiring rather more than 200,000 observations. Preparations for the work of observation were immediately commenced; and, by the time of the next report in 1869, considerable progress had been made.

In the report for this year, the provisional places of a catalogue of 539 fundamental stars were published. This catalogue is composed of two parts. The list of hauptsterne consists of 336 stars to the fourth magnitude, observed at Pulkova by Wagner with the large transit instrument, and by Gyldén with the Ertel vertical circle. The list of zusat-sterne consists of 203 stars fainter than the fourth magnitude. As the details of the work in the formation of the provisional places of the stars of this list are not given

in the report, it is not quite clear upon what authority they rest. The work assigned to the Pulkova observatory by the zone commission was the exact determination of the places of the stars of this list. The observations were undertaken by Gromadski with the Repsold meridian circle. In accordance with the plan adopted, each star was observed eight times, — four times in each position of the instrument. The observations were differential with respect to the hauptsterne.

The results were published by Struve in 1876; and the places there given were used in the first reduction of the Harvard-college observations for 1874–75, and perhaps in some other cases.

About this time a change seems to have been made in the original plan with respect to the formation of the final catalogue of fundamental stars, of which I have been unable to find a clear account. The original intention was to make the positions depend entirely upon the observations at Pulkova. The zone commission established by the gesellschaft, however, committed the formation of this catalogue to Auwers; and it is to him that we owe the most complete and the most perfect catalogue of fundamental stars yet published. The Pulkova system for 1865 was adopted as the basis; but, in order to obtain greater freedom from accidental errors for individual stars, the final catalogue was obtained by combining with the Pulkova series, the Greenwich observations from 1836 to 1876, the Harvard-college observations for 1871–72, the Leipsic observations, in declination only, between 1866 and 1870, and the Leiden observations in declination between 1864 and 1870. Before this combination was made, however, these observations were all reduced to the Pulkova system.

The following observatories have taken part in the zone observations: —

Observatories.	Limits of zones in declination.	Observatories.	Limits of zones in declination.
Nicolajeff . . .	− 2° to + 1°	Lund	+35° to +40°
Albany	+ 1 " + 5	Bonn	+40 " +50
Leipsic	+ 4 " +10	Harvard college .	+50 " +55
Leipsic	+10 " +15	Helsingfors . .	+55 " +60
Berlin	+15 " +25	Christiana . . .	+65 " +70
Cambridge (Eng.)	+25 " +30	Dorpat	+70 " +75
Leiden	+30 " +35	Kasan	+75 " +80

The zone between −2° and +1° was originally undertaken at Palermo, that between +1° and +4° at Neuchâtel, that between +4° and +10° at Mannheim, and that between +35° and +40° at Chicago.

In the latter case, the great fire at Chicago crippled the resources of the observatory to such an extent, that Safford was compelled to relinquish the work, which was at that time quite far advanced.

The chief items of interest in connection with this work are found in the following tabular statement: —

[*Table omitted.*]

Attention was called, at an early date, to the importance of continuing the survey of the northern heavens beyond the southern limit fixed by Argelander. The preparation necessary for the execution of this work consisted in the extension of the Durchmusterung to the tropic of Capricorn. This was undertaken by Schönfeld at Leipsic.

In the report to the gesellschaft at the meeting held at Stockholm in 1877, he has given an account of this work, in which he stated that it was sufficiently near completion to invite the consideration of the question of the meridian circle determinations of the places of stars to the ninth magnitude. The lack of southern fundamental stars whose positions were well determined was still a hinderance to the immediate commencement of the work. Relatively more stars of this class are required than in the northern observations, in order to eliminate the inequalities due to refraction. Schönfeld stated, that, while the burden of the determination of the places of these southern fundamental stars must rest mainly upon southern observations, it seemed necessary to connect them with the Pulkova system by a connecting link (mittelglied), through observations at some observatory well situated for this purpose. At this meeting Sande Bakhuysen, at Leiden, gave notice of intention to take part in this work. Gyldén urged the importance of securing the co-operation of Melbourne; and Peters suggested the advantage of securing Washington as an additional 'mean term' (V. J. S. 1877, p. 265).

The next reference to this work is contained in the vierteljahrsschrift for 1881, xv. p. 270. A list of 303 southern stars is here given, whose exact places were at that time being determined at Leiden and at the Cape of Good Hope. This list was selected by Schönfeld and Sande Bakhuysen, in a way to meet the requirements referred to in previous discussions.

A final catalogue of 83 southern fundamental stars by Auwers appears in this number of the vierteljahrsschrift. The places depend upon the same authorities as for the northern stars, with the addition of the Cape of Good Hope catalogue for 1860, Williamstown, Melbourne for 1870, and Harvard college (Safford) for 1864. For stars not observed at Pulkova, the general catalogue of Yarnall (1858–1861), and the Washington observations, with the new meridian circle between 1872 and 1875, were employed. As in the case of the northern stars, these observations are all reduced to the Pulkova system for 1865. It is understood that the co-ordinates of the list of 303 stars are to depend upon this extension of the general system of publication xiv. to the limits required by the southern Durchmusterung of Schönfeld.

It would be surprising if all the conditions of success were fulfilled in the first execution of a work having the magnitude, and involving the difficulties, of the scheme of observations undertaken under the auspices of the gesellschaft. The extent of the discordances which are to be expected between the results obtained by different observers can only be ascertained when the observations by which the different zones are to be connected have been reduced. Each observer extended the working-list of his own zone 10′ north and south; and it is expected that a sufficient number of observations of this kind has been made to determine the systematic relations

existing between the co-ordinates of each zone with those of its neighbor.

It is probable, however, that the experience of Gill will be repeated on a larger scale. In 1878 he solicited the co-operation of astronomers in the determination of the co-ordinates of twenty-eight stars, which he desired to employ in the reduction of his heliometer observations of the planet Mars for the purpose of obtaining the solar parallax. The results obtained at twelve observatories of the first class are published in vol. xxxix. p. 99, of the monthly notices of the Royal astronomical society. Notwithstanding the fact that the final values obtained at each observatory depend upon several observations, the average difference between the least and the greatest results, obtained by different observers for each star, is 0.24^s. in right ascension and 2.3″ in declination. In four cases the difference in right ascension exceeds 3.0, and in four cases the difference in declination exceeds 3.0.″

Even after the results are reduced to a homogeneous system, the following outstanding deviations from a mean system are found: —

Authority.	$\Delta\ \alpha$	$\Delta\ \delta$	Authority.	$\Delta\ \alpha$	$\Delta\ \delta$
	s.	″		s.	″
Koenigsberg . .	+.005	−0.71	Leiden	−.053	−0.19
Melbourne . . .	+.026	−0.49	Paris	+.055	+0.01
Pulkova. . . .	+.005	+0.36	Washington . .	−.120	+0.78
Leipsic	+.049	+0.40	Harvard college,	−.072	+0.09
Greenwich. . .	+.009	−0.56	Cordoba . . .	−.032	−0.20
Berlin	+.044	+0.67	Oxford	+.076	+0.21

The observations of a second list of twelve stars, one-half of the number being comparatively bright, and the remaining half faint, showed no marked improvement, either with respect to the magnitude of errors which could be classed as accidental, or in regard to the systematic deviations from a mean system.

This discussion revealed one source of discordance which will doubtless affect the zone observations; viz., the difference between right ascensions determined by the eye-and-ear method, and those determined with the aid of the chronograph.

The programme of the gesellschaft makes no provision for the elimination of errors which depend upon the magnitude of the stars observed; but special observations have been undertaken at several observatories for the purpose of defining the relation between the results for stars of different magnitudes. At Harvard-college observatory, the direct effect of a reduction of the magnitude has been ascertained by reducing the aperture of the telescope by means of diaphragms. Beside this, the observations have been arranged in such a manner that an error depending upon the magnitude can be derived from an investigation of the observations upon two successive nights.

At Leiden, at Albany, and perhaps at other observatories, the effect of magnitude has been determined by observations through wire gauze. But notwithstanding all the precautions which have been taken in the observations, and which may be taken in the reductions, it will undoubtedly be found that the final results obtained will involve errors which cannot be entirely eliminated.

In the experience of the speaker, two other sources of error have been detected. It has been found, that there is a well-defined equation between the observations, which is a function of the amount, and the character of the illumination of the field of the telescope. It has also been found that observations made under very unfavorable atmospheric conditions differ systematically from those made under favorable conditions. When the seeing was noted as very bad, it is found that the observed right ascensions are about $.08^s$ too great, and that the observed declinations are about 0.8″ too great.

There are doubtless other sources of error which the discussion of the observations will bring to light. The effect of the discovery of these and other errors will probably be to hasten the repetition of the zone observations under a more perfect scheme, framed in such a manner as to cover all the deficiencies which experience has revealed, or may yet reveal. One would not probably go far astray in naming the year 1900 as the mean epoch of the new survey. If the observations are again repeated in 1950, sufficient data will then have been accumulated for at least an approximate determination of the laws of siderial motion.

What is the present state of our knowledge upon this subject? It can be safely said that it is very limited. First of all, it cannot be affirmed that there is a sidereal system in the sense in which we speak of the solar system. In the case of the solar system, we have a central sun about which the planets and their satellites revolve in obedience to laws which are satisfied by the hypothesis of universal gravitation. Do the same laws pervade the inter-stellar spaces? Is the law of gravitation indeed universal? What physical connection exists between the solar system and the unnumbered and innumerable stars which form the galaxy of the heavens? Do these stars form a system which has its own laws of relative rest and motion? or is the solar system a part of the stupendous whole? Does the solar system receive its laws from the sidereal system? or has Kepler indeed pierced the depths of the universe in the discovery of the laws which gave him immortality? Are we to take the alternative stated by Ball, — either that our sidereal system is not an entirely isolated object, or its bodies must be vastly more numerous or more massive than even our most liberal interpretation of observations would seem to warrant? Are we to conclude, for example, that stars like 1830 Groombridge and α Centauri, "after having travelled from an infinitely great distance on one side of the heavens, are now passing through our system for the first and only time, and that after leaving our system they will retreat again into the depths of space to a distance which, for any thing we can tell, may be practically regarded as infinite"? Can we assert with Newcomb, that in all probability the stars do not

form a stable system in the sense in which we say that the solar system is stable, — that the stars of this system do not revolve around definite attractive centres? Admitting that the solar system is moving through space, can we at the present moment even determine whether that motion is rectilinear, or curved, to say nothing of the laws which govern that motion? How much of truth is there in the conjectures of Wright, Kant, Lambert, and Mitchel, or even in the more serious conclusions of Moedler, that the Alcyone of the Pleiades is the central sun about which the solar system revolves?

These are questions which, if solved at all, must be solved by a critical study of observations of precision accumulated at widely separated epochs of time. The first step in the solution has been taken in the systematic survey of the northern heavens undertaken by the gesellschaft, and in the survey of the southern heavens at Cordoba by Dr. Gould. The year 1875 is the epoch about which are grouped the data which, combined with similar data for an epoch not earlier than 1950, will go far towards clearing up the doubts which now rest upon the question of the direction and the amount of the solar motion in space; and it cannot be doubted that our knowledge of the laws which connect the sidereal with the solar system will be largely increased through this investigation. The basis of this knowledge must be the observed proper motions of a selected list of stars, so exactly determined that the residual mean error shall not affect the results derived; or, failing in this, of groups of stars symmetrically distributed over the visible heavens, sufficient in number to affect an elimination of the accidental errors of observation, without disturbing the equilibrium of the general system.

For an investigation of this kind, a complete system of zone observations, at widely separated intervals, will afford the necessary data, if the following conditions are fulfilled.

First: The proper motions must be derived by a method which does not involve an exact knowledge of the constants of precession. In every investigation with which I am acquainted, the derived proper motions are functions of this element.

Second: The general system of proper motions derived must be free from systematic errors. Errors of this class may be introduced either through the periodic errors inherent in the system of fundamental stars employed in the reduction of the zone observations, or in a change in the constants of precession. It is in this respect that the utmost precaution will be required. If from any cause errors of even small magnitude are introduced into the general system of proper motions at any point, the effect of these errors upon the values of the co-ordinates at any future epoch will be directly proportional to the interval elapsed. We can, therefore, compute the exact amount of the accumulated error for any given time.

When this test is applied to the fundamental stellar systems independently determined by Auwers, Safford, Boss, and Newcomb, we find the following deviations *inter se* at the end of a century.

	Maximum mean deviation in a century.		Maximum systematic deviation in a century.	
	$\Delta\alpha$	$\Delta\delta$		
Auwers *minus* Safford	-0.22^s	$+0.2''$	0.23^s	$1.1''$
Auwers *minus* Boss	–	+0.8	–	2.1
Auwers *minus* Newcomb	−0.09	+0.8	0.06	2.2

It is the common impression, that both the direction and the amount of the motion of the solar system in space are now well established. The conclusions of Struve upon this point are stated in such explicit language that it is not surprising that this impression exists. He says, "The motion of the solar system in space is directed to a point in the celestial sphere situated on the right line which joins the two stars measured from π and ω Herculis. The velocity of this motion is such that the sun, with the whole cortége of bodies depending on him, advances annually in the direction indicated, through a space equal to one hundred and fifty-four million miles."

It must be admitted that there is a general agreement in the assignment by different investigators of the co-ordinates of the solar apex. This will be seen from the following tabular values.

Authorities.	Right ascension.	Declination.
Herschel, 1783	257° 00′	+25° 00′
Prevost	230 00	+25 00
Klugel, 1789	260 00	+27 00
Herschel, 1805	245 52	+49 38
Argelander, 1837	257 49	+28 50
Lundahl	252 24	+14 26
Struve	261 22	+37 36
Galloway	260 01	+34 23
Mädler	261 38	+39 54
Airy	256 54	+34 29
	261 29	+26 44
Dunkin	261 14	+32 55
	263 44	+25 00

In estimating the value which should be attached to these results, several considerations must be taken into account.

(*a*) All of the results except those of Galloway depend practically upon the same authorities at one epoch, viz., upon Brodley.

(*b*) The deviations *inter se* probably result, in a large measure, from the systematic errors inherent in one or both of the fundamental systems from which the proper motions were derived. For example, Lundahl employed Pond as one of his authorities, and it is in Pond's catalogue that the most decided periodic errors exist.

(*c*) Brot in 1812, Bessel in 1818, and Airy in 1860, reached the conclusion that the *certainty* of the movement of the solar system towards a given point in the heavens could not be affirmed.

(*d*) The problem is indirect. In the case of a mem-

ber of the solar system, exact data will determine the exact position in orbit at a given time; but here we have neither exact data, nor can we employ trigonometrical methods in the solution. We simply find that the observed proper motions are probably somewhat better reconciled under the hypothesis of an assumed position of the apex of the solar motion. The method of investigation employed by Safford, who has of late years given much attention to this subject, consists in assuming a system of co-ordinates for the pole of the solar motion, from which is determined the direction each star would have if its own proper motions were zero. Comparing this direction with the observed direction as indicated by the observed proper motion, equations of condition are formed from which a correction is found to the assumed position of the apex, by the methods of least squares.

It must always be kept in mind, that the quantities with which we must deal in this investigation are exceedingly minute, and that the accidental errors of observation are at any time liable to lead to illusory results. The weak link in the chain of Mädler's reasoning is to be found here. I think we can assume 0.2″ as the limit of precision in the absolute determination of the co-ordinates of any star, however great the number of observations upon which it depends. Beyond this limit it is impossible to go, in the present date of instrumental astronomy.

It is safe to say, that there is not a single star in the heavens whose co-ordinates are known with certainty within this limit. Do not misunderstand me. Doubtless there are many stars in which the error will at some future time be found to fall within this limit. The law of probabilities requires this, if the maximum limit falls within 1″. But who is prepared to select a particular star, and say that the absolute position of this star in space cannot be more than 0.2″ in error?

e. At present an arbitrary hypothesis is necessary in the discussion of the problem. Airy assumed that the relative distances of the stars are proportional to their magnitudes; and he found slightly different results according to different modes of treatment. Safford assumed that the distances are, at least approximately, in inverse proportion to the magnitude of the proper motions. The general result of his investigations, up to this point, is, that there is some hope of using the solar motion as a base, to advance our knowledge of stellar distances. Later investigations have been made by De Ball, but the details have not yet come to hand. It is understood, however, that his results coincide in a general way with those previously obtained.

It is clear from this brief review, that we have here a field of investigation worthy of the highest powers of the astronomer. The first step has been taken in the survey of the heavens carried on under the auspices of the gesellschaft. It remains for the astronomers of the present generation to solve the difficulties which now environ the problem, and prepare the way for a more perfect scheme of observation in the next century.

PAPERS READ BEFORE SECTION A.

The total solar eclipse of May 6, 1883.

BY EDWARD S. HOLDEN, OF WASHBURN OBSERVATORY, MADISON, WIS.

THIS eclipse had the longest totality of any which has been observed.

An expedition was sent by the National academy of sciences and the U. S. coast-survey jointly, under direction of a committee from the former. Expenses were met by an appropriation of $5,000 by congress and by the National academy of sciences from a fund left by Professor Watson. The navy department also placed the U. S. steamer Hartford at the disposal of the academy, to transport the expedition from Peru to Caroline island, where the eclipse was to be observed, and thence to Honolulu.

The efforts of Mr. Rockwell to provide money by private subscription for this undertaking, though directly unsuccessful, prepared the way by drawing public attention.

Professor Young was the chairman of the committee of the National academy of sciences: it was at one time hoped that he would take charge of the observing-party, but this proved impracticable. The reports of different members of the party are to be submitted to the National academy of sciences in November. Mr. Holden has, however, permission of the academy to present an account of the observation before the American association. It is understood that the present is not by any means a final report. This especially applies to the observations of Dr. Hastings, from which that gentleman concludes that the solar corona is chiefly a phenomenon due to the diffraction of the solar light at the moon's limb. The computations to demonstrate this are not yet at hand, but are to be completed in a few weeks.

The American party consisted of Edward S. Holden, director of Washburn observatory, Madison, Wis.; Charles S. Hastings, professor of physics in the Johns Hopkins university, Baltimore, Md.; Charles H. Rockwell, Tarrytown, N.Y.; E. D. Preston, aid U. S. coast and geodetic survey, Washington, D.C.; Winslow Upton, U.S. signal-office, Washington, D.C.; and Ensign S. J. Brown, U.S.N., U. S. naval observatory, Washington, D.C.

The original six members of the party were joined, on April 20, by four volunteer observers, all officers of the U. S. ship Hartford: these were Lieut. E. F. Qualtrough, U.S.N.; Passed assistant-surgeon W. S. Dixon, U.S.N.; Midshipman W. S. Fletcher, U.S.N.; and Midshipman J. G. Doyle, U.S.N.

On March 11 the party was strengthened by the joining (at Colon) of the two English gentlemen who were sent out by the Royal society of London to make photographic observations of the eclipse, under instructions from J. Norman Lockyer, Esq., F.R.S., and Capt. W. de W. Abney, R.E., of the science and art department of the South Kensington museum. These were H. A. Lawrance, London, Eng., and C. Ray Woods, London, Eng.

During the stay of the party on Caroline island

(April 21 to May 9), ten petty officers and men of the Hartford remained, and rendered very intelligent assistance.

In all, the party on the island consisted of twenty-two persons.

After giving details of the proceedings of the expedition, its arrival, and the preparations for the eclipse, Mr. Holden states, as to the event itself, that the following atmospheric conditions prevailed: The sky proved clear at first contact, cloudy at intervals till near totality, clear during totality except a slight haze in its first minutes, cloudy a few minutes after third contact, and finally clear at fourth contact.

The meteorological observations (for which due credit is given to the members of the party that had them in charge) are noteworthy. In two weeks, April 25 to May 9, twenty showers were recorded; but the rainfall in each was very small, the total in the two weeks being about 8 inches. Half of this fell during the only considerable disturbance of the weather, which took place May 4, when it rained from midnight to 9.50 A.M.

The barometer was notably uniform. Its diurnal movements were plainly marked; the maxima being at 9 A.M. and P.M., the minima at 3 A.M. and P.M. The indications of the thermometer were very constant. The daily range was 9.3°, the highest reading 89.3°, the lowest 72.4°, the daily maximum at noon, the minimum at 6 A.M. The relative humidity ranged from 70 per cent at midday to 84 in early morning, and at no time fell below 61. The island lies in the region of the south-east trades, but the wind (which was very steady) blew constantly between north and east. The average velocity of the wind was 6.05 miles; the largest during twenty-four hours was 212 miles, the least 59 miles; the highest velocity, registered in a squall, was 16 miles per hour.

The botanical and zoölogical observations are not yet ready for publication. During the voyage a series of observations was made by Mr. Upton on southern variable stars. Dr. Hastings and Mr. Holden, while on the island, discovered twenty-three new double stars, a list of which has appeared in SCIENCE.

In preparing for the eclipse, Mr. Holden assigned to each observer a single duty, not requiring him to move from one instrument to another. The excellent photographic apparatus, prepared under the direction of Prof. W. Harkness of the U.S. naval observatory, was not used: the entire field of photography was left to the English party accompanying our own, and to the French party under M. Janssen, who were very successful in photographing the corona.

The combination of polariscope and telescope was used, but not with successful results, the apparatus proving unsuitable. Dr. W. S. Dixon, who attended to a telescopic examination of the details of the inner corona, will report on the same separately, giving a drawing of the corona. With the spectroscope, the chief point of observation was as to the relative lengths of the line 1474 east and west of the sun. At second contact, this line was 12′ longitude east and 3′ west. The length of 1474 east diminished, while 1474 west increased. At mid-totality these were equal. Before the third contact, the appearances were reversed: 1474 west was longer and brighter than 1474 east.

At the beginning of totality, the lines C, D_3, F, and (near G) were seen brilliant but very short. At mid-eclipse the spectrum was deliberately examined. On a continuous spectrum, two lines only were seen: 1474 *bright*, and the D line *dark*. C, E, b, F, were certainly wanting. Near the end of totality, C, D_3, and F appeared again, very short. Five seconds after second contact, four curved lines were seen, — C, D_3, 1474, F. A light cloud passed over the sun; and on its disappearance the spectrum showed a small line, of about one-third the height of the others, between 1474 and F. One hundred seconds after second contact, three coronal rings took the place of the lines: they were red, yellowish-green, and green, and are supposed to be C, D_3, and 1474. Two hundred seconds after second contact, the red ring was decidedly the brightest, and it continued to increase in brightness during sixty seconds. Two hundred and ninety seconds after second contact, the four curved lines, C, D_3, 1474, F, appeared. The reversal of the bright lines at third contact was observed. The change was instantaneous, or nearly so. The reversal of the Fraunhofer lines was not seen. The only bright line seen for the first 190 seconds was 1474. A dark line was seen, which was probably D.

Mr. Rockwell, using a Rutherford grating and a narrow slip tangential to the limb, reported that 1474 K was not seen until a minute and a half had passed. It was followed 4′ or 5′ west of the limb, twice; and it was seen only on the western side of the moon. Two green lines were also seen, each brighter and broader than 1474, but much shorter.

Due credit is given by Mr. Holden to each of the observers of the party. His own observations were confined to a search for the planet Vulcan, reported to exist by Professors Watson and Swift. Mr. Holden's search continued during the whole of totality (five minutes and twenty-five seconds), with a six-inch telescope with a power of 44 and field of 57′ in declination. He saw every star on the map which he had previously published in SCIENCE (Feb. 23, 1883), down to the sixth magnitude, inclusive, except the thirty-sixth magnitude stars nearest to the sun; and he saw only these stars. One of the stars of the map was of the same magnitude as Watson's 'Vulcan.' This was a conspicuous object. No star half so bright as this could possibly have escaped observation. Mr. Holden is therefore confident that Vulcan did not exist within the limits swept over. Mr. Holden also determined the direction of the motion of the diffraction bands before and after totality. This was an observation which he could not make successfully in Colorado in 1878, and which he believes has not been before made.

A new method of investigating the flexure corrections of a meridian circle.

BY PROF. W. A. ROGERS OF CAMBRIDGE, MASS.

THE error due to refraction, the flexure of the circle itself, and the astronomical flexure, the three being functions in themselves, are most prolific errors respecting flexures of a meridian circle.

The theory which suggested itself was arrived at from the use on the telescope of a level of a different construction from any the author had ever seen. He had been a disbeliever in a level, but this device converted him into an advocate of the level. The level tube is attached to a plate, and the plate attached to the cube of the telescope. Then set the telescope at the north point, and reverse it to the south, reading the circle north and south. It would be much better were the point fixed upon a ring so that it can be readily placed at any inclination.

Results of tests with the almacantar, in time and latitude.

BY S. C. CHANDLER, OF CAMBRIDGE, MASS.

THE instrument which has been named the 'almacantar' was described and figured in a paper presented to the association at its meeting in 1880. In its general nature it is an equal altitude instrument. A hollow rectangular trough containing mercury revolves horizontally on an upright central pillar. The trough contains a float which is perfectly free to obtain equilibrium, while it is constrained to revolve with the trough. The float carries a telescope which turns on a horizontal axis, and can be clamped at any desired altitude. When this instrument is revolved on its vertical axis, any given point in the field of view describes a horizontal small circle, or almacantar, in the heavens. The transits of stars over a series of horizontal lines will thus afford means of determining the altitude of the instrument, the error of the clock, the latitude or the declinations of stars, by a proper distribution of the observations in azimuth.

A higher degree of accuracy is attainable by this instrument than by a transit or a zenith telescope of same size. The author's comparison of results is as follows: The probable error of a single star in determining the clock error is only $\pm 0.05^s$ or $\pm 0.06^s$. With a transit instrument of the same size, the quantity is not less than $\pm 0.08^s$. With the almacantar the probable error in determining the latitude of a single star is $\pm 0.55''$, including the error of the star's place. This is about equal to the probable error of a pair of stars by Talcott's method, with the larger telescopes of the United-States coast-survey.

The instrument was a small one, — $1\frac{3}{4}$ inches aperture and 25 inches focus. It was constructed for experiment only, in a provisional way, at a cost of $150. There are obvious defects in design and construction: when these are remedied, the error can be much reduced.

A series of observations with this instrument are given by the author, for the latitude of a pier about 80 feet north of the Harvard-college observatory. The value obtained by averaging these is $0.7''$ less than given by Professor Peirce in his discussion of the prime vertical transit observations taken by the Messrs. Bond, and adopted as the standard value of the latitude of the observatory. The author concludes that Professor Peirce's value is too large by fully three-quarters of a second. By way of proof the author gives a series of observations on the five stars used by Professor Peirce. These are compared with those of Auwers and Boss, and the correction of the hitherto accepted value of the latitude now indicated by the almacantar is thereby confirmed.

The clock errors of two nights selected at random, as given by the almacantar, were exhibited by the author. The results both in time and latitude would be considered satisfactory with an ordinary instrument of two or three times the size. The almacantar can be made much larger than the one under trial, certainly of five or six inches aperture, with corresponding increase of precision along with greater optical power. Its mechanical construction is simple, and reduces the sources of error. Thus in the older instruments there are involved: 1°. The accurate construction of parts, as of pivots, level, graduated circles. 2°. Fixity of mounting, to avoid a shifting of the instrumental plane. 3°. Rigidity of the instrument itself, to secure constancy of collimation and flexure. In the almacantar only the last condition has to be satisfied, and it is by far the easiest of the three to be attained mechanically.

The author regards the principle of flotation adopted as being as delicate an indication of the direction of gravity as is obtained by the spirit-level.

The almacantar gives promise of a new instrumental resource in the higher practical astronomy. It is competent to deal with the most delicate problems. It will evade some of the minute sources of error that still cling to meridian instruments. Especially, it furnishes a method for obviating difficulties, hitherto regarded as almost insuperable, connected with flexure and refraction, in observations with the meridian circle.

Internal contacts in transits of the inferior planets.

BY J. R. EASTMAN, OF WASHINGTON, D.C.

THE author began by reviewing the different values obtained in observing transits of Venus, and by computations thereon since 1761. Eventually it became certain that the differences of these values depended chiefly upon the computer's interpretation of the observer's record. The phenomenon known as the 'black drop' began to be considered as an element in the calculation. Stone regarded it as a necessary phenomenon. He gave an explanation of its origin, and stated that the moment when a dark ligament appears to connect the apparent limbs of the sun and Venus is the time of *real* internal contact. The second phase, when the limbs of Venus and the sun appear in contact, Stone says, is 'the *apparent* internal contact.'

In 1876 M. André, the astronomer in charge of the French expedition to Nouméa, in 1874, announced that "the bridge, black ligament, or black drop, as it is variously called, is a necessary phenomenon under certain circumstances, and not merely accidental." He noticed, however, that "it is always possible to get rid of the ligament, and reduce the phenomenon to geometrical constants, either (*a*) by reducing sufficiently the intensity of the source of light, or augmenting the absorbing power of the dark glass employed; or (*b*) by covering the object-glass with a dark diaphragm composed of rings alternately full and empty, all very thin, and bearing a certain proportion to the focal length of the lens."

These results and opinions of M. André were not generally known at the time of the transit of Mercury in 1878; although his theories were confirmed by his observations at Utah at that date, the results being published by him in 1881. The black drop was seen and recognized in 1878 by many observers of Mercury; some evidently regarding their success in finding it as a proof of accuracy of observation, others apologizing for failing to perceive the phenomenon.

The author of this paper regards it as noteworthy, that every observer, so far as ascertained, who got, by means of shade-glasses, the best definition of the sun's limbs, with an illumination less than the eye could easily bear, did not see any trace of the black drop. Before seeing any account of M. André's experiments, and having given little attention to his deductions announced by Father Perry, the author became independently convinced, after observation of the transit of Mercury in 1878, that the theory of a *necessary* black drop was fallacious.

While, in 1874, many American observers perceived the black drop, none appear to have seen it, among the eight American parties organized by the transit-of-Venus commission of 1882.

The paper winds up with an account of the observations of contact at the transit-of-Venus station at Cedar Keys, Fla., last December. The observation of first contact was prevented by a cloud covering a part of the sun's disk. On the disappearance of the cloud, the illumination was reduced by a sliding shade-glass, till easily endured by the eye. The definition of the sun's limb was perfect. When haze or cirri interfered, a less density of shade-glass was permitted; the steadiness and definition of the limb remaining, and that of Venus being 'all that could be desired,' with no modification, at the edge of the disk, of its dense black color.

Before the second contact, the entire disk of Venus was visible for several minutes. The portion beyond the sun's disk was bordered by a narrow line of light much less bright than the limb of the sun, and of a lighter tint. About one minute before contact, the apparent motion of the cusps of the sun, as they closed around the planet, noticeably increased, although the movement was perfectly steady. The cusps swept around the planet in a line of sunlight of the same tint as adjacent parts of the sun. This line was as narrow as could be seen with the power used, — 216 diameters, — and was free from tremors or pulsations. There was no agitation in the limb of either body near the point of contact, no trace of black drop, ligament, or band, no change of tint or color on the limb of Venus, and no indication of any clinging of the limbs. The contact was as easily, and perhaps as accurately, observed as the transit of a star within 8° of the pole, under the best conditions. The uncertainty of noting the time of the visible contact could not have been greater than three-tenths of a second. The phenomena at the third contact were similar to those at the second, but, of course, in a reversed order.

In conclusion, the author urges his belief, founded upon his own experience as well as on study of the work of other observers, that, with a properly arranged telescope and shade-glass, no observer need have trouble from any phase of the 'black drop.' To attain this end satisfactorily, the observer of contacts must have no other purpose in view than such observation. The study of any branch of solar physics, or searching for some new thing, may, and probably will, detract from the accuracy of his work, which should be confined to obtaining the record of a good definition of the sun's limb, as a reference-point in the passage of the limb of the planet.

An improved method of producing a dark-field illumination of lines ruled upon glass.

BY PROF. W. A. ROGERS OF CAMBRIDGE, MASS.

BY repeated and careful tests the author found that by letting the light, which is held at an angle of 45°, into the telescope, and then splitting the rays by means of two opposite mirrors, throwing them on the horizontal line, an almost perfect light is secured. Thereby it becomes practicable to see with distinctness stars of the smaller magnitudes upon a dark field.

Other astronomers present expressed a preference for the use of red light. Professor Rogers claimed that his method was better for minute observation.

Physical phenomena on the planet Jupiter.

BY G. W. HOUGH OF CHICAGO, ILL.

THE rapid motion of revolution of the planet, by changing the positions of the markings on the surface to our line of sight, makes great apparent differences in their shapes and sizes. This has perhaps been the occasion of reports of sudden and great changes upon the surface. The changes are not sudden, but are gradual; and many of the features are permanent. Minor changes are constantly in progress in the equatorial belts. The author recently observed the belt drifting down toward the red spot; but although it partly surrounded it, they did not coalesce, and the spot forced a scallop into the belt, — a very curious phenomenon. The author saw a satellite pass over this red spot, though the satellites are not visible when on the white part of the disk. He had also had a chance to compare shadows of satellites on the disk and on the spot, and both are dark. The red spot has seemingly retrogaded during the past four years; that is to say, the rotation of Jupiter has seemingly

increased from 9 h. 55 m. 33 s., to 9 h. 55 m. 38 s. The future observer should attend more carefully to what he sees, and theorize afterward.

French observations on the solar eclipse of May 6, 1883.

BY DR. J. JANSSEN OF PARIS, FRANCE.

A LETTER from the French astronomer Dr. Janssen, who passed through this country on his return from an eclipse expedition, was addressed by him for the use of the association to Professor Eastman, who translated it, and read the translation in Section A. It was thus entered as one of the papers. Dr. Janssen says, —

"The principal object of the observations was the study of the dark rays in the corona. The visibility of these rays depends more on the light-power of the instrument than upon the perfection of the images. At first the ordinary brilliant rays which the corona presents were recognized; but what was new, and more complete than ever expected, was that the background of the coronal spectrum presented the Fraunhofer's spectrum. All the dark rays were theoretically visible. Phenomena were observed, which indicated that there were some portions of the corona which reflected, much more abundantly than others, the light emanating from the solar sphere: this would indicate the existence of cosmic matter circulating around the sun. The rings of Rispighi were not found arranged symmetrically around the sun. The light of the corona was strongly and radially polarized. All these things were associated with the problem of circumsolar cosmic matter. The observations went to show that no important intra-mercurial planet exists."

Some hitherto undeveloped properties of squares.

BY O. S. WESTCOTT OF CHICAGO, ILL.

THE paper began by ascribing due credit to a method for obtaining squares and square roots, described by Samuel Emerson in 1865. The principles and details of that method were briefly summarized. Mr. Westcott then stated the general principles of his own method, which is very expeditious. He first shows that the tens and units figures of all perfect squares of numbers, from 26 to 49 inclusive, are the same as the tens and units figures of perfect squares of numbers from 24 to 1 inclusive. A table is presented as follows:

$$(24)^2 = 576, \text{ add } 100, = 676 = (26)^2$$
$$(23)^2 = 529, \text{ add } 200, = 729 = (27)^2$$
$$(22)^2 = 484, \text{ add } 300, = 784 = (28)^2$$

and so on, to

$$(1)^2 = 1, \text{ add } 2400, = 2401 = (49)^2$$

To determine the square of any number between 25 and 50, find the corresponding number below 25, and augment its square by the number of hundreds indicated by its remoteness from 25. Or, more conveniently, take the excess above 25 as hundreds, and augment by the square of what the number lacks of 50.

Thus: $(43)^2 = (43 - 25) \,.\, 100 + (50 - 43)^2$
$\quad = \quad 1800 \quad + \quad 49 \quad = 1849$

Conversely: To obtain the square root of 1764. The root is plainly between 25 and 50. The tens and units figures indicate 8. Therefore the square root of 1764 is $50 - 8 = 42$.

It is further observable, that the tens and units figures of perfect squares of numbers from 51 to 99 inclusive, are the same as the tens and units figures of the squares of numbers from 49 to 1 inclusive. Since $4 \times$ any number of hundreds + 25, 50, or 75, gives an exact number of hundreds, it follows that the tens and units figures of the squares of numbers less than 25 represent all the possible combinations of figures in those orders of units for *all* square numbers. The terminations of all perfect square numbers are 22 in all: viz., 00, 01, 04, 09, 16, 21, 24, 25, 29, 36, 41, 44, 49, 56, 61, 64, 69, 76, 81, 84, 89, 96.

The following rule is then deduced: To square any number from 50 to 100, take twice the excess above 50 as hundreds, and augment by the square of what the number lacks of 100.

Thus: $(89)^2 = 200\,(89 - 50) + (100 - 89)^2$
$\quad = \quad 7800 \quad + \quad 121 \quad = 7921$

Conversely, $\sqrt{3249}$: The root is plainly between 50 and 60; the tens and units figures indicate 7; therefore $\sqrt{3249} = 50 + 7 = 57$.

For greater convenience it is noted, that in such a case as $\sqrt{7921}$ the root is $50 + 39$ or $100 - 11$, and it is easier to use the latter form. That is, if the root is in the fourth quarter of the hundred, subtract the number indicated by the tens and units from 100, and the difference is the root. Thus $\sqrt{8281} = 100 - 9 = 91$.

To square any number from 100 to 200, take four times the excess above 100 as hundreds, and augment by the square of what the number lacks of 200.

To square any number from 125 to 250, take one-half the excess above 125 as thousands, and augment by what the number lacks of 250.

By a series of steps of this character, the author gives methods for squaring higher numbers, and conversely for obtaining their square roots. A choice of methods is also indicated. The facility which was obtained by such means was deftly illustrated on the blackboard by the author, who in a few seconds performed such exploits as raising 5 to the 16th power, and then showed in detail the processes which he had mentally executed. The paper sets forth the reason for each rule, deducing it from the usual binomial theorem, with almost obvious simplicity.

The demonstrations were received by the section with hearty applause. In response to an inquiry, Mr. Westcott stated, that he had been very successful in teaching this method in classes, about a tenth of his pupils becoming rapid experts in the methods of solution, which were especially useful in handling quadratic equations, and determining at a glance whether a given number is or is not a perfect square.

PROCEEDINGS OF SECTION B. — PHYSICS.

ADDRESS OF H. A. ROWLAND OF BALTIMORE, MD., VICE-PRESIDENT OF SECTION B, AUG. 15, 1883.

A PLEA FOR PURE SCIENCE.[1]

THE question is sometimes asked us as to the time of year we like the best. To my mind, the spring is the most delightful; for nature then recovers from the apathy of winter, and stirs herself to renewed life. The leaves grow, and the buds open, with a suggestion of vigor delightful to behold; and we revel in this ever-renewed life of nature. But this cannot always last. The leaves reach their limit; the buds open to the full, and pass away. Then we begin to ask ourselves whether all this display has been in vain, or whether it has led to a bountiful harvest.

So this magnificent country of ours has rivalled the vigor of spring in its growth. Forests have been levelled, and cities built, and a large and powerful nation has been created on the face of the earth. We are proud of our advancement. We are proud of such cities as this, founded in a day upon a spot over which, but a few years since, the red man hunted the buffalo. But we must remember that this is only the spring of our country. Our glance must not be backward; for however beautiful leaves and blossoms are, and however marvellous their rapid increase, they are but leaves and blossoms after all. Rather should we look forward to discover what will be the outcome of all this, and what the chance of harvest. For if we do this in time, we may discover the worm which threatens the ripe fruit, or the barren spot where the harvest is withering for want of water.

I am required to address the so-called physical section of this association. Fain would I speak pleasant words to you on this subject; fain would I recount to you the progress made in this subject by my countrymen, and their noble efforts to understand the order of the universe. But I go out to gather the grain ripe to the harvest, and I find only tares. Here and there a noble head of grain rises above the weeds; but so few are they, that I find the majority of my countrymen know them not, but think that they have a waving harvest, while it is only one of weeds after all. American science is a thing of the future, and not of the present or past; and the proper course of one in my position is to consider what must be done to create a science of physics in this country, rather than to call telegraphs, electric lights, and such conveniences, by the name of science. I do not wish to underrate the value of all these things: the progress of the world depends on them, and he is to be honored who cultivates them successfully. So also the cook who invents a new and palatable dish for the table benefits the world to a certain degree; yet we do not dignify him by the name of a chemist. And yet it is not an uncommon thing, especially in American newspapers, to have the *applications* of science confounded with pure science; and some obscure American who steals the ideas of some great mind of the past, and enriches himself by the application of the same to domestic uses, is often lauded above the great originator of the idea, who might have worked out hundreds of such applications, had his mind possessed the necessary element of vulgarity. I have often been asked, which was the more important to the world, pure or applied science. To have the applications of a science, the science itself must exist. Should we stop its progress, and attend only to its applications, we should soon degenerate into a people like the Chinese, who have made no progress for generations, because they have been satisfied with the applications of science, and have never sought for reasons in what they have done. The reasons constitute pure science. They have known the application of gunpowder for centuries; and yet the reasons for its peculiar action, if sought in the proper manner, would have developed the science of chemistry, and even of physics, with all their numerous applications. By contenting themselves with the fact that gunpowder will explode, and seeking no farther, they have fallen behind in the progress of the world; and we now regard this oldest and most numerous of nations as only barbarians. And yet our own country is in this same state. But we have done better; for we have taken the science of the old world, and applied it to all our uses, accepting it like the rain of heaven, without asking whence it came, or even acknowledging the debt of gratitude we owe to the great and unselfish workers who have given it to us. And, like the rain of heaven, this pure science has fallen upon our country, and made it great and rich and strong.

To a civilized nation of the present day, the applications of science are a necessity; and our country has hitherto succeeded in this line, only for the reason that there are certain countries in the world where pure science has been and is cultivated, and where the study of nature is considered a noble pursuit. But such countries are rare, and those who wish to pursue pure science in our own country must be prepared to face public opinion in a manner which requires much moral courage. They must be prepared to be looked down upon by every successful inventor whose shallow mind imagines that the only pursuit of mankind is wealth, and that he who obtains most has best succeeded in this world. Everybody can comprehend a million of money; but how few can comprehend any advance in scientific theory, especially in its more abstruse portions! And this, I believe, is one of the causes of the small number of persons who have ever devoted themselves to work of the higher order in any human pursuit. Man is a gregarious animal, and depends very much, for his happiness, on the sympathy of those around him; and it is

[1] In using the word 'science,' I refer to physical science, as I know nothing of natural science. Probably my remarks will, however, apply to both, but I do not know.

rare to find one with the courage to pursue his own ideals in spite of his surroundings. In times past, men were more isolated than at present, and each came in contact with a fewer number of people. Hence that time constitutes the period when the great sculptures, paintings, and poems were produced. Each man's mind was comparatively free to follow its own ideals, and the results were the great and unique works of the ancient masters. To-day the railroad and the telegraph, the books and newspapers, have united each individual man with the rest of the world: instead of his mind being an individual, a thing apart by itself, and unique, it has become so influenced by the outer world, and so dependent upon it, that it has lost its originality to a great extent. The man who in times past would naturally have been in the lowest depths of poverty, mentally and physically, to-day measures tape behind a counter, and with lordly air advises the naturally born genius how he may best bring his outward appearance down to a level with his own. A new idea he never had, but he can at least cover his mental nakedness with ideas imbibed from others. So the genius of the past soon perceives that his higher ideas are too high to be appreciated by the world: his mind is clipped down to the standard form; every natural offshoot upwards is repressed, until the man is no higher than his fellows. Hence the world, through the abundance of its intercourse, is reduced to a level. What was formerly a grand and magnificent landscape, with mountains ascending above the clouds, and depths whose gloom we cannot now appreciate, has become serene and peaceful. The depths have been filled, and the heights levelled, and the wavy harvests and smoky factories cover the landscape.

As far as the average man is concerned, the change is for the better. The average life of man is far pleasanter, and his mental condition better, than before. But we miss the vigor imparted by the mountains. We are tired of mediocrity, the curse of our country. We are tired of seeing our artists reduced to hirelings, and imploring congress to protect them against foreign competition. We are tired of seeing our countrymen take their science from abroad, and boast that they here convert it into wealth. We are tired of seeing our professors degrading their chairs by the pursuit of applied science instead of pure science; or sitting inactive while the whole world is open to investigation; lingering by the wayside while the problem of the universe remains unsolved. We wish for something higher and nobler in this country of mediocrity, for a mountain to relieve the landscape of its monotony. We are surrounded with mysteries, and have been created with minds to enjoy and reason to aid in the unfolding of such mysteries. Nature calls to us to study her, and our better feelings urge us in the same direction.

For generations there have been some few students of science who have esteemed the study of nature the most noble of pursuits. Some have been wealthy, and some poor; but they have all had one thing in common, — the love of nature and its laws. To these few men the world owes all the progress due to applied science, and yet very few ever received any payment in this world for their labors.

Faraday, the great discoverer of the principle on which all machines for electric lighting, electric railways, and the transmission of power, must rest, died a poor man, although others and the whole world have been enriched by his discoveries. And such must be the fate of the followers in his footsteps for some time to come.

But there will be those in the future who will study nature from pure love, and for them higher prizes than any yet obtained are waiting. We have but yet commenced our pursuit of science, and stand upon the threshold wondering what there is within. We explain the motion of the planet by the law of gravitation; but who will explain how two bodies, millions of miles apart, tend to go toward each other with a certain force?

We now weigh and measure electricity and electric currents with as much ease as ordinary matter, yet have we made any approach to an explanation of the phenomenon of electricity? Light is an undulatory motion, and yet do we know what it is that undulates? Heat is motion, yet do we know what it is that moves? Ordinary matter is a common substance, and yet who shall fathom the mystery of its internal constitution?

There is room for all in the work, and the race has but commenced. The problems are not to be solved in a moment, but need the best work of the best minds, for an indefinite time.

Shall our country be contented to stand by, while other countries lead in the race? Shall we always grovel in the dust, and pick up the crumbs which fall from the rich man's table, considering ourselves richer than he because we have more crumbs, while we forget that he has the cake, which is the source of all crumbs? Shall we be swine, to whom the corn and husks are of more value than the pearls? If I read aright the signs of the times, I think we shall not always be contented with our inferior position. From looking down we have almost become blind, but may recover. In a new country, the necessities of life must be attended to first. The curse of Adam is upon us all, and we must earn our bread.

But it is the mission of applied science to render this easier for the whole world. There is a story which I once read, which will illustrate the true position of applied science in the world. A boy, more fond of reading than of work, was employed, in the early days of the steam-engine, to turn the valve at every stroke. Necessity was the mother of invention in his case: his reading was disturbed by his work, and he soon discovered that he might become free from his work by so tying the valve to some movable portion of the engine, as to make it move its own valve. So I consider that the true pursuit of mankind is intellectual. The scientific study of nature in all its branches, of mathematics, of mankind in its past and present, the pursuit of art, and the cultivation of all that is great and noble in the world, — these are the highest occupation of mankind. Commerce, the applications of science, the accumulation

of wealth, are necessities which are a curse to those with high ideals, but a blessing to that portion of the world which has neither the ability nor the taste for higher pursuits.

As the applications of science multiply, living becomes easier, the wealth necessary for the purchase of apparatus can better be obtained, and the pursuit of other things beside the necessities of life becomes possible.

But the moral qualities must also be cultivated in proportion to the wealth of the country, before much can be done in pure science. The successful sculptor or painter naturally attains to wealth through the legitimate work of his profession. The novelist, the poet, the musician, all have wealth before them as the end of a successful career. But the scientist and the mathematician have no such incentive to work: they must earn their living by other pursuits, usually teaching, and only devote their surplus time to the true pursuit of their science. And frequently, by the small salary which they receive, by the lack of instrumental and literary facilities, by the mental atmosphere in which they exist, and, most of all, by their low ideals of life, they are led to devote their surplus time to applied science or to other means of increasing their fortune. How shall we, then, honor the few, the very few, who, in spite of all difficulties, have kept their eyes fixed on the goal, and have steadily worked for pure science, giving to the world a most precious donation, which has borne fruit in our greater knowledge of the universe and in the applications to our physical life which have enriched thousands and benefited each one of us? There are also those who have every facility for the pursuit of science, who have an ample salary and every appliance for work, yet who devote themselves to commercial work, to testifying in courts of law, and to any other work to increase their present large income. Such men would be respectable if they gave up the name of professor, and took that of consulting chemists or physicists. And such men are needed in the community. But for a man to occupy the professor's chair in a prominent college, and, by his energy and ability in the commercial applications of his science, stand before the local community in a prominent manner, and become the newspaper exponent of his science, is a disgrace both to him and his college. It is the deathblow to science in that region. Call him by his proper name, and he becomes at once a useful member of the community. Put in his place a man who shall by precept and example cultivate his science, and how different is the result! Young men, looking forward into the world for something to do, see before them this high and noble life, and they see that there is something more honorable than the accumulation of wealth. They are thus led to devote their lives to similar pursuits, and they honor the professor who has drawn them to something higher than they might otherwise have aspired to reach.

I do not wish to be misunderstood in this matter. It is no disgrace to make money by an invention, or otherwise, or to do commercial scientific work under some circumstances. But let pure science be the aim of those in the chairs of professors, and so prominently the aim that there can be no mistake. If our aim in life is wealth, let us honestly engage in commercial pursuits, and compete with others for its possession. But if we choose a life which we consider higher, let us live up to it, taking wealth or poverty as it may chance to come to us, but letting neither turn us aside from our pursuit.

The work of teaching may absorb the energies of many; and, indeed, this is the excuse given by most for not doing any scientific work. But there is an old saying, that where there is a will there is a way. Few professors do as much teaching or lecturing as the German professors, who are also noted for their elaborate papers in the scientific journals. I myself have been burdened down with work, and know what it is; and yet I here assert that all *can* find time for scientific research if they desire it. But here, again, that curse of our country, mediocrity, is upon us. Our colleges and universities seldom call for first-class men of reputation, and I have even heard the trustee of a well-known college assert that no professor should engage in research because of the time wasted! I was glad to see, soon after, by the call of a prominent scientist to that college, that the majority of the trustees did not agree with him.

That teaching is important, goes without saying. A successful teacher is to be respected; but if he does not lead his scholars to that which is highest, is he not blameworthy? We are, then, to look to the colleges and universities of the land for most of the work in pure science which is done. Let us therefore examine these latter, and see what the prospect is.

One, whom perhaps we may here style a practical follower of Ruskin, has stated that while in this country he was variously designated by the title of captain, colonel, and professor. The story may or may not be true, but we all know enough of the customs of our countrymen not to dispute it on general principles. All men are born equal: some men are captains, colonels, and professors, and therefore all men are such. The logic is conclusive; and the same kind of logic seems to have been applied to our schools, colleges, and universities. I have before me the report of the commissioner of education for 1880. According to that report, there were 389,[1] or say, in round numbers, 400 institutions, calling themselves colleges or universities, in our country! We may well exclaim that ours is a great country, having more than the whole world beside. The fact is sufficient. The whole earth would hardly support such a number of first-class institutions. The curse of mediocrity must be upon them, to swarm in such numbers. They must be a cloud of mosquitoes, instead of eagles as they profess. And this becomes evident on further analysis. About one-third aspire to the name of university; and I note one called by that name which has two professors and 18 students, and another having three teachers and 12 students! And these instances are not unique, for the number of small institutions and schools which call themselves universities is very great. It is difficult to

[1] 364 reported on, and 25 not reported.

decide from the statistics alone the exact standing of these institutions. The extremes are easy to manage. Who can doubt that an institution with over 800 students, and a faculty of 70, is of a higher grade than those above cited having 10 or 20 students and two or three in the faculty? Yet this is not always true; for I note one institution with over 500 students which is known to me personally as of the grade of a high school. The statistics are more or less defective, and it would much weaken the force of my remarks if I went too much into detail. I append the following tables, however, of 330 so-called colleges and universities:—

218	had	from	0	to	100	students.
88	"	"	100	"	200	"
12	"	"	200	"	300	"
6	"	"	300	"	500	"
6	over		500			

Of 322 so-called colleges and universities:—

206	had	0	to	10	in the	faculty.
99	"	10	"	20	"	"
17	"	20	or over		"	"

If the statistics were forthcoming,—and possibly they may exist,—we might also get an idea of the standing of these institutions and their approach to the true university idea, by the average age of the scholars. Possibly also the ratio of number of scholars to teachers might be of some help. All these methods give an approximation to the present standing of the institutions. But there is another method of attacking the problem, which is very exact, but it only gives us the *possibilities* of which the institution is capable. I refer to the wealth of the institution. In estimating the wealth, I have not included the value of grounds and buildings, for this is of little importance, either to the present or future standing of the institution. As good work can be done in a hovel as in a palace. I have taken the productive funds of the institution as the basis of estimate. I find:—

234	have below	$500,000.
8	" between	$500,000 and $1,000,000.
8	" over	$1,000,000.

There is no fact more firmly established, all over the world, than that the higher education can never be made to pay for itself. Usually the cost to a college, of educating a young man, very much exceeds what he pays for it, and is often three or four times as much. The higher the education, the greater this proportion will be; and a university of the highest class should anticipate only a small accession to its income from the fees of students. Hence the test I have applied must give a true representation of the possibilities in every case. According to the figures, only 16 colleges and universities have $500,000 or over of invested funds, and only one-half of these have $1,000,000 and over. Now, even the latter sum is a very small endowment for a college; and to call any institution a university which has less than $1,000,000, is to render it absurd in the face of the world. And yet more than 100 of our institutions, many of them very respectable colleges, have abused the word 'university' in this manner. It is to be hoped that the endowment of the more respectable of these institutions may be increased, as many of them deserve it; and their unfortunate appellation has probably been repented of long since.

But what shall we think of a community that gives the charter of a university to an institution with a total of $20,000 endowment, two so-called professors, and 18 students! or another with three professors, 12 students, and a total of $27,000 endowment, mostly invested in buildings! And yet there are very many similar institutions; there being 16 with three professors or less, and very many indeed with only four or five.

Such facts as these could only exist in a democratic country, where pride is taken in reducing every thing to a level. And I may also say, that it can only exist in the early days of such a democracy; for an intelligent public will soon perceive that calling a thing by a wrong name does not change its character, and that truth, above all things, should be taught to the youth of the nation.

It may be urged, that all these institutions are doing good work in education; and that many young men are thus taught, who could not afford to go to a true college or university. But I do not object to the education,—though I have no doubt an investigation would disclose equal absurdities here,—for it is aside from my object. But I do object to lowering the ideals of the youth of the country. Let them know that they are attending a school, and not a university; and let them know that above them comes the college, and above that the university. Let them be taught that they are only half-educated, and that there are persons in the world by whose side they are but atoms. In other words, let them be taught the truth.

It may be that some small institutions are of high grade, especially those which are new; but who can doubt that more than two-thirds of our institutions calling themselves colleges and universities are unworthy of the name? Each one of these institutions has so-called professors, but it is evident that they can be only of the grade of teachers. Why should they not be so called? The position of teacher is an honored one, but is not made more honorable by the assumption of a false title. Furthermore, the multiplication of the title, and the ease with which it can be obtained, render it scarcely worth striving for. When the man of energy, ability, and perhaps genius is rewarded by the same title and emoluments as the commonplace man with the modicum of knowledge, who takes to teaching, not because of any aptitude for his work, but possibly because he has not the energy to compete with his fellow-men in business, then I say one of the inducements for first-class men to become professors is gone.

When work and ability are required for the position, and when the professor is expected to keep up with

the progress of his subject, and to do all in his power to advance it, and when he is selected for these reasons, then the position will be worth working for, and the successful competitor will be honored accordingly. The chivalric spirit which prompted Faraday to devote his life to the study of nature may actuate a few noble men to give their life to scientific work; but, if we wish to cultivate this highest class of men in science, we must open a career for them worthy of their efforts.

Jenny Lind, with her beautiful voice, would have cultivated it to some extent in her native village; yet who would expect her to travel over the world, and give concerts for nothing ? and how would she have been able to do so if she had wished? And so the scientific man, whatever his natural talents, must have instruments and a library, and a suitable and respectable salary to live upon, before he is able to exert himself to his full capacity. This is true of advance in all the higher departments of human learning, and yet something more is necessary. It is not those in this country who receive the largest salary, and have positions in the richest colleges, who have advanced their subject the most: men receiving the highest salaries, and occupying the professor's chair, are to-day doing absolutely nothing in pure science, but are striving by the commercial applications of their science to increase their already large salary. Such pursuits, as I have said before, are honorable in their proper place; but the duty of a professor is to advance his science, and to set an example of pure and true devotion to it which shall demonstrate to his students and the world that there is something high and noble worth living for. Money-changers are often respectable men, and yet they were once severely rebuked for carrying on their trade in the court of the temple.

Wealth does not constitute a university, buildings do not: it is the men who constitute its faculty, and the students who learn from them. It is the last and highest step which the mere student takes. He goes forth into the world, and the height to which he rises has been influenced by the ideals which he has consciously or unconsciously imbibed in his university. If the professors under whom he has studied have been high in their profession, and have themselves had high ideals; if they have considered the advance of their particular subject their highest work in life, and are themselves honored for their intellect throughout the world, — the student is drawn toward that which is highest, and ever after in life has high ideals. But if the student is taught by what are sometimes called good teachers, and teachers only, who know little more than the student, and who are often surpassed and even despised by him, no one can doubt the lowered tone of his mind. He finds that by his feeble efforts he can surpass one to whom a university has given its highest honor; and he begins to think that he himself is a born genius, and the incentive to work is gone. He is great by the side of the molehill, and does not know any mountain to compare himself with.

A university should not only have great men in its faculty, but have numerous minor professors and assistants of all kinds, and should encourage the highest work, if for no other reason than to encourage the student to his highest efforts.

But, assuming that the professor has high ideals, wealth such as only a large and high university can command is necessary to allow him the fullest development.

And this is specially so in our science of physics. In the early days of physics and chemistry, many of the fundamental experiments could be performed with the simplest apparatus. And so we often find the names of Wollaston and Faraday mentioned as needing scarcely any thing for their researches. Much can even now be done with the simplest apparatus; and nobody, except the utterly incompetent, need stop for want of it. But the fact remains, that one can only be free to investigate in all departments of chemistry and physics, when he not only has a complete laboratory at his command, but a friend to draw on for the expenses of each experiment. That simplest of the departments of physics, namely, astronomy, has now reached such perfection that nobody can expect to do much more in it without a perfectly equipped observatory; and even this would be useless without an income sufficient to employ a corps of assistants to make the observations and computations. But even in this simplest of physical subjects, there is great misunderstanding. Our country has very many excellent observatories: and yet little work is done in comparison, because no provision has been made for maintaining the work of the observatory; and the wealth which, if concentrated, might have made one effective observatory which would prove a benefit to astronomical science, when scattered among a half-dozen, merely furnishes telescopes for the people in the surrounding region to view the moon with. And here I strike the keynote of at least one need of our country, if she would stand well in science; and the following item which I clip from a newspaper will illustrate the matter: —

"The eccentric old Canadian, Arunah Huntington, who left $200,000 to be divided among the public schools of Vermont, has done something which will be of little practical value to the schools. Each district will be entitled to the insignificant sum of $10, which will not advance much the cause of education."

Nobody will dispute the folly of such a bequest, or the folly of filling the country with telescopes to look at the moon, and calling them observatories. How much better to concentrate the wealth into a few parcels, and make first-class observatories and institutions with it!

Is it possible that any of our four hundred colleges and universities have love enough of learning to unite with each other and form larger institutions? Is it possible that any have such a love of truth that they are willing to be called by their right name? I fear not; for the spirit of expectation, which is analogous to the spirit of gambling, is strong in the American breast, and each institution which now, except in name, slumbers in obscurity, expects in

time to bloom out into full prosperity. Although many of them are under religious influence, where truth is inculcated, and where men are taught to take a low seat at the table in order that they may be honored by being called up higher, and not dishonored by being thrust down lower, yet these institutions have thrust themselves into the highest seats, and cannot probably be dislodged.

But would it not be possible to so change public opinion that no college could be founded with a less endowment than say $1,000,000, or no university with less than three or four times that amount? From the report of the commissioner of education, I learn that such a change is taking place; that the tendency towards large institutions is increasing, and that it is principally in the west and south-west that the multiplication of small institutions with big names is to be feared most, and that the east is almost ready for the great coming university.

The total wealth of the four hundred colleges and universities in 1880 was about $40,000,000 in buildings, and $43,000,000 in productive funds. This would be sufficient for one great university of $10,000,000, four of $5,000,000, and twenty-six colleges of $2,000,000 each. But such an idea can of course never be carried out. Government appropriations are out of the question, because no political trickery must be allowed around the ideal institution.

In the year 1880 the private bequests to all schools and colleges amounted to about $5,500,000; and, although there was one bequest of $1,250,000, yet the amount does not appear to be phenomenal. It would thus seem that the total amount was about five million dollars in one year, of which more than half is given to so-called colleges and universities. It would be very difficult to regulate these bequests so that they might be concentrated sufficiently to produce an immediate result. But the figures show that generosity is a prominent feature of the American people, and that the needs of the country only have to be appreciated to have the funds forthcoming. We must make the need of research and of pure science felt in the country. We must live such lives of pure devotion to our science, that all shall see that we ask for money, not that we may live in indolent ease at the expense of charity, but that we may work for that which has advanced and will advance the world more than any other subject, both intellectually and physically. We must live such lives as to neutralize the influence of those who in high places have degraded their profession, or have given themselves over to ease, and do nothing for the science which they represent. Let us do what we can with the present means at our disposal. There is not one of us who is situated in the position best adapted to bring out all his powers, and to allow him to do most for his science. All have their difficulties, and I do not think that circumstances will ever radically change a man. If a man has the instinct of research in him, it will always show itself in some form. But circumstances may direct it into new paths, or may foster it so that what would otherwise have died as a bud now blossoms and ripens into the perfect fruit.

Americans have shown no lack of invention in small things; and the same spirit, when united to knowledge and love of science, becomes the spirit of research. The telegraph-operator, with his limited knowledge of electricity and its laws, naturally turns his attention to the improvement of the only electrical instrument he knows any thing about; and his researches would be confined to the limited sphere of his knowledge, and to the simple laws with which he is acquainted. But as his knowledge increases, and the field broadens before him, as he studies the mathematical theory of the subject, and the electro-magnetic theory of light loses the dim haze due to distance, and becomes his constant companion, the telegraph-instrument becomes to him a toy, and his effort to discover something new becomes research in pure science.

It is useless to attempt to advance science until one has mastered the science: he must step to the front before his blows can tell in the strife. Furthermore, I do not believe anybody can be thorough in any department of science, without wishing to advance it. In the study of what is known, in the reading of the scientific journals, and the discussions therein contained of the current scientific questions, one would obtain an impulse to work, even though it did not before exist. And the same spirit which prompted him to seek what was already known, would make him wish to know the unknown. And I may say that I never met a case of thorough knowledge in my own science, except in the case of well-known investigators. I have met men who talked well, and I have sometimes asked myself why they did not do something; but further knowledge of their character has shown me the superficiality of their knowledge. I am no longer a believer in men who could do something if they would, or would do something if they had a chance. They are impostors. If the true spirit is there, it will show itself in spite of circumstances.

As I remarked before, the investigator in pure science is usually a professor. He must teach as well as investigate. It is a question which has been discussed in late years, as to whether these two functions would better be combined in the same individual, or separated. It seems to be the opinion of most, that a certain amount of teaching is conducive, rather than otherwise, to the spirit of research. I myself think that this is true, and I should myself not like to give up my daily lecture. But one must not be overburdened. I suppose that the true solution, in many cases, would be found in the multiplication of assistants, not only for the work of teaching but of research. Some men are gifted with more ideas than they can work out with their own hands, and the world is losing much by not supplying them with extra hands. Life is short: old age comes quickly, and the amount one pair of hands can do is very limited. What sort of shop would that be, or what sort of factory, where one man had to do all the work with his own hands? It is a fact in nature, which no democracy can change, that men are *not* equal, — that some have brains, and some hands. And no idle

talk about equality can ever subvert the order of the universe.

I know of no institution in this country where assistants are supplied to aid directly in research. Yet why should it not be so? And even the absence of assistant professors and assistants of all kinds, to aid in teaching, is very noticeable, and must be remedied before we can expect much.

There are many physical problems, especially those requiring exact measurements, which cannot be carried out by one man, and can only be successfully attacked by the most elaborate apparatus, and with a full corps of assistants. Such are Regnault's experiments on the fundamental laws of gases and vapors, made thirty or forty years ago by aid from the French government, and which are the standards to this day. Although these experiments were made with a view to the practical calculation of the steam-engine, yet they were carried out in such a broad spirit that they have been of the greatest theoretical use. Again, what would astronomy have done without the endowments of observatories? By their means, that science has become the most perfect of all branches of physics, as it should be from its simplicity. There is no doubt, in my mind, that similar institutions for other branches of physics, or, better, to include the whole of physics, would be equally successful. A large and perfectly equipped physical laboratory with its large revenues, its corps of professors and assistants, and its machine-shop for the construction of new apparatus, would be able to advance our science quite as much as endowed observatories have astronomy. But such a laboratory should not be founded rashly. The value will depend entirely on the physicist at its head, who has to devise the plan, and to start it into practical working. Such a man will always be rare, and cannot always be obtained. After one had been successfully started, others could follow; for imitation requires little brains.

One could not be certain of getting the proper man every time, but the means of appointment should be most carefully studied so as to secure a good average. There can be no doubt that the appointment should rest with a scientific body capable of judging the highest work of each candidate.

Should any popular element enter, the person chosen would be either of the literary-scientific order, or the dabbler on the outskirts who presents his small discoveries in the most theatrical manner. What is required is a man of depth, who has such an insight into physical science that he can tell when blows will best tell for its advancement.

Such a grand laboratory as I describe does not exist in the world, at present, for the study of physics. But no trouble has ever been found in obtaining means to endow astronomical science. Everybody can appreciate, to some extent, the value of an observatory; as astronomy is the simplest of scientific subjects, and has very quickly reached a position where elaborate instruments and costly computations are necessary to further advance. The whole domain of physics is so wide that workers have hitherto found enough to do. But it cannot always be so, and the time has even now arrived when such a grand laboratory should be founded. Shall our country take the lead in this matter, or shall we wait for foreign countries to go before? They will be built in the future, but when and how is the question.

Several institutions are now putting up laboratories for physics. They are mostly for teaching, and we can expect only a comparatively small amount of work from most of them. But they show progress; and, if the progress be as quick in this direction as in others, we should be able to see a great change before the end of our lives.

As stated before, men are influenced by the sympathy of those with whom they come in contact. It is impossible to immediately change public opinion in our favor; and, indeed, we must always seek to lead it, and not be guided by it. For pure science is the pioneer who must not hover about cities and civilized countries, but must strike into unknown forests, and climb the hitherto inaccessible mountains which lead to and command a view of the promised land, — the land which science promises us in the future; which shall not only flow with milk and honey, but shall give us a better and more glorious idea of this wonderful universe. We must create a public opinion in our favor, but it need not at first be the general public. We must be contented to stand aside, and see the honors of the world for a time given to our inferiors; and must be better contented with the approval of our own consciences, and of the very few who are capable of judging our work, than of the whole world beside. Let us look to the other physicists, not in our own town, not in our own country, but in the whole world, for the words of praise which are to encourage us, or the words of blame which are to stimulate us to renewed effort. For what to us is the praise of the ignorant? Let us join together in the bonds of our scientific societies, and encourage each other, as we are now doing, in the pursuit of our favorite study; knowing that the world will some time recognize our services, and knowing, also, that we constitute the most important element in human progress.

But danger is also near, even in our societies. When the average tone of the society is low, when the highest honors are given to the mediocre, when third-class men are held up as examples, and when trifling inventions are magnified into scientific discoveries, then the influence of such societies is prejudicial. A young scientist attending the meetings of such a society soon gets perverted ideas. To his mind, a molehill is a mountain, and the mountain a molehill. The small inventor or the local celebrity rises to a greater height, in his mind, than the great leader of science in some foreign land. He gauges himself by the molehill, and is satisfied with his stature; not knowing that he is but an atom in comparison with the mountain, until, perhaps, in old age, when it is too late. But, if the size of the mountain had been seen at first, the young scientist would at least have been stimulated in his endeavor to grow.

We cannot all be men of genius; but we can, at

least, point them out to those around us. We may not be able to benefit science much ourselves; but we can have high ideals on the subject, and instil them into those with whom we come in contact. For the good of ourselves, for the good of our country, for the good to the world, it is incumbent on us to form a true estimate of the worth and standing of persons and things, and to set before our own minds all that is great and good and noble, all that is most important for scientific advance, above the mean and low and unimportant.

It is very often said, that a man has a right to his opinion. This might be true for a man on a desert island, whose error would influence only himself. But when he opens his lips to instruct others, or even when he signifies his opinions by his daily life, then he is directly responsible for all his errors of judgment or fact. He has no right to think a molehill as big as a mountain, nor to teach it, any more than he has to think the world flat, and teach that it is so. The facts and laws of our science have *not* equal importance, neither have the men who cultivate the science achieved equal results. One thing is greater than another, and we have no right to neglect the order. Thus shall our minds be guided aright, and our efforts be toward that which is the highest.

Then shall we see that no physicist of the first class has ever existed in this country, that we must look to other countries for our leaders in that subject, and that the few excellent workers in our country must receive many accessions from without before they can constitute an American science, or do their share in the world's work.

But let me return to the subject of scientific societies. Here American science has its hardest problem to contend with. There are very many local societies dignified by high-sounding names, each having its local celebrity, to whom the privilege of describing some crab with an extra claw, which he found in his morning ramble, is inestimable. And there are some academies of science, situated at our seats of learning, which are doing good work in their locality. But distances are so great that it is difficult to collect men together at any one point. The American association, which we are now attending, is not a scientific academy, and does not profess to be more than a gathering of all who are interested in science, to read papers and enjoy social intercourse. The National academy of sciences contains eminent men from the whole country, but then it is only for the purpose of advising the government freely on scientific matters. It has no building, it has no library; and it publishes nothing except the information which it freely gives to the government, which does nothing for it in return. It has not had much effect directly on American science; but the liberality of the government in the way of scientific expeditions, publications, etc., is at least partly due to its influence, and in this way it has done much good. But it in no way takes the place of the great Royal society, or the great academies of science at Paris, Berlin, Vienna, St. Petersburgh, Munich, and, indeed, all the European capitals and large cities. These, by their publications, give to the young student, as well as the more advanced physicist, models of all that is considered excellent; and to become a member is one of the highest honors to which he can aspire, while to write a memoir which the academy considers worthy to be published in its transactions excites each one to his highest effort.

The American academy of sciences in Boston is perhaps our nearest representation of this class of academies, but its limitation of membership to the State deprives it of its national character.

But there is another matter which influences the growth of our science.

As it is necessary for us still to look abroad for our highest inspiration in pure science, and as science is not an affair of one town or one country, but of the whole world, it becomes us all to read the current journals of science and the great transactions of foreign societies, as well as those of our own countries. These great transactions and journals should be in the library of every institution of learning in the country, where science is taught. How can teachers and professors be expected to know what has been discovered in the past, or is being discovered now, if these are not provided? Has any institution a right to mentally starve the teachers whom it employs, or the students who come to it? There can be but one answer to this; and an institution calling itself a university, and not having the current scientific journals upon its table or the transactions of societies upon its library-shelves, is certainly not doing its best to cultivate all that is best in this world.

We call this a free country, and yet it is the only one where there is a direct tax upon the pursuit of science. The low state of pure science in our country may possibly be attributed to the youth of the country; but a direct tax, to prevent the growth of our country in that subject, cannot be looked upon as other than a deep disgrace. I refer to the duty upon foreign books and periodicals. In our science, no books above elementary ones have ever been published, or are likely to be published, in this country; and yet every teacher in physics must have them, not only in the college library, but on his own shelves, and must pay the government of this country to allow him to use a portion of his small salary to buy that which is to do good to the whole country. All freedom of intercourse which is necessary to foster our growing science is thus broken off; and that which might, in time, relieve our country of its mediocrity, is nipped in the bud by our government, which is most liberal when appealed to directly on scientific subjects.

One would think that books in foreign languages might be admitted free; but to please the half-dozen or so workmen who reprint German books, not scientific, our free intercourse with that country is cut off. Our scientific associations and societies must make themselves heard in this matter, and show those in authority how the matter stands.

In conclusion, let me say once more, that I do not believe that our country is to remain long in its present position. The science of physics, in whose applications our country glories, is to arise among us,

and make us respected by the nations of the world. Such a prophecy may seem rash with regard to a nation which does not yet do enough physical work to support a physical journal. But we know the speed with which we advance in this country: we see cities springing up in a night, and other wonders performed at an unprecedented rate. And now we see physical laboratories being built, we see a great demand for thoroughly trained physicists, who have not shirked their mathematics, both as professors and in so-called practical life; and perhaps we have the feeling, common to all true Americans, that our country is going forward to a glorious future, when we shall lead the world in the strife for intellectual prizes as we now do in the strife for wealth.

But if this is to be so, we must not aim low. The problems of the universe cannot be solved without labor: they cannot be attacked without the proper intellectual as well as physical tools; and no physicist need expect to go far without his mathematics. No one expects a horse to win in a great and long race who has not been properly trained; and it would be folly to attempt to win with one, however pure his blood and high his pedigree, without it. The problems we solve are more difficult than any race: the highest intellect cannot hope to succeed without proper preparation. The great prizes are reserved for the greatest efforts of the greatest intellects, who have kept their mental eye bright and flesh hard by constant exercise. Apparatus can be bought with money, talents may come to us at birth; but our mental tools, our mathematics, our experimental ability, our knowledge of what others have done before us, all have to be obtained by work. The time is almost past, even in our own country, when third-rate men can find a place as teachers, because they are unfit for every thing else. We wish to see brains and learning, combined with energy and immense working-power, in the professor's chair; but, above all, we wish to see that high and chivalrous spirit which causes one to pursue his idea in spite of all difficulties, to work at the problems of nature with the approval of his own conscience, and not of men before him. Let him fit himself for the struggle with all the weapons which mathematics and the experience of those gone before him can furnish, and let him enter the arena with the fixed and stern purpose to conquer. Let him not be contented to stand back with the crowd of mediocrity, but let him press forward for a front place in the strife.

The whole universe is before us to study. The greatest labor of the greatest minds has only given us a few pearls; and yet the limitless ocean, with its hidden depths filled with diamonds and precious stones, is before us. The problem of the universe is yet unsolved, and the mystery involved in one single atom yet eludes us. The field of research only opens wider and wider as we advance, and our minds are lost in wonder and astonishment at the grandeur and beauty unfolded before us. Shall we help in this grand work, or not? Shall our country do its share, or shall it still live in the almshouse of the world?

PAPERS READ BEFORE SECTION B.

Determination of the relation between the imperial yard and the metre of the archives.

BY WILLIAM A. ROGERS OF CAMBRIDGE, MASS.

THIS paper was a continuation of one upon the same subject presented at the Montreal meeting. The mean result of the determinations up to that time was as follows: Imperial yard + 3.37015 inches = Metre des archives.

The writer stated at that time, that he should not like to be held to a very strict account with regard to the last decimal figure, or even the last two decimal figures, on account of the difficulty of obtaining the requisite data.

Since the meeting last year, additional data have been obtained. In February of the present year, a combined yard and metre was received from Paris. The yard was compared with the imperial yard, in 1880, by Mr. Chaney, the warden of the imperial standards. During the interval between 1880 and February of the present year, this metre has received repeated comparisons with the metre of the International bureau, under the direction of Dr. Pernet. According to his report, this metre is 310 mikrons too short at 0° centigrade; for the same temperature, the yard was found by Mr. Chaney to be 20.7 mikrons too short.

Comparing the metre and the yard upon this bar with the bronze yard and metre described at Montreal, and combining the results with those previously found, the relation was found as follows: Imperial yard + 3.37039 inches = Metre des archives.

The magnetophone, or the modification of the magnetic field by the rotation of a perforated metallic disk.[1]

BY PROF. H. S. CARHART OF EVANSTON, ILL.

THE experiments of Bell, Preece, and others, on the radiaphone, suggested the possibility of interrupting, or at least periodically modifying, the lines of force proceeding from the poles of a magnet, by means of a disk of sheet-iron, perforated with a series of equidistant holes, and rotated so that the holes should pass directly in front of the magnetic pole. It is well known that the armature placed on the poles of a permanent magnet diminishes the strength of the external field of force by furnishing superior facilities for the formation of polarized chains of particles from pole to pole. This is the case even when the armature does not touch the poles, but is in close proximity to them.

If a piece of sheet-iron be placed over the poles of a magnet without touching, and magnetic curves be developed on paper above the iron, they will be found to exhibit less intense and less sharply defined magnetic action than when the sheet-iron is removed. If, however, a small hole be drilled directly over each magnetic pole, the screening action of the sheet-iron is modified in much the same way as when a hole is

[1] This paper will shortly be published in SCIENCE in full.

made in a screen opaque to light; for the developed curves show distinctly the outline of the holes. If, therefore, the sheet-iron in the form of a circular plate, pierced with a number of holes, be rapidly rotated between the poles of a magnet and small induction bobbins, the action of the magnet on the core of the bobbins will be periodically modified, because of the passing holes: and hence induced currents will flow through a circuit including the bobbin. A disk of sheet iron was pierced with two circles of quarter-inch holes concentric with the disk, the number of holes in the two circles being thirty-two and sixty-four respectively. On one side of the disk was placed a horseshoe magnet with its poles very near the rows of holes; on the other side were arranged two corresponding induction bobbins. The circuit was completed through a telephone and either bobbin at pleasure. Upon rotating the disk rapidly, a clear musical sound was produced in the telephone, the pitch rising with the rapidity of rotation. Moreover, the bobbin opposite the circle of sixty-four holes gave the octave above the other, and each gave a note of the same pitch as was produced by blowing a stream of air through the corresponding holes.

Magnetic survey of Missouri.

BY F. E. NIPHER OF ST. LOUIS, MO.

IN the spring of 1878 a survey of Missouri was begun, which was expected to determine all points in regard to terrestrial magnetism: 160 points have been covered. The work was undertaken under private auspices, most of the money tendered unasked, and the work has been carried on successfully until the present time. The first three years were spent in making a preliminary survey. In the early part of the survey we labored under great difficulties, because I supposed that the lines of equal value, laid down upon the observations given in the coast-survey charts, were substantially correct; so that time was frequently lost in repeating values at stations left behind, in order to be certain that no error had been committed. But when we settled down to the conclusion that we really knew nothing about the matter, we had very much less trouble. At first, intensity determinations were made at each station; but in later years, since the magnets have proved so satisfactory, the plan was adopted of making absolute determinations only at regular intervals during the summer. The temperature corrections for the magnet were made twice, — once in 1878, and once two years ago, — and they agreed very closely with each other.

The dip circle was a large one, such as was formerly much used, and which was found to be an excellent instrument, though rather clumsy to carry. The charts which have been prepared show what the results were. In a former communication to the association at Cincinnati, I suggested an explanation of the peculiar flexures of the isogonic lines, as being due to earth-currents which seemed to be deflected by the moist river-valleys. The map upon which that hypothesis was based represented observations taken over the entire state. By re-determination we have found that those observations were all correct; but more detailed work shows that this explanation is not admissible. There is no explanation of the fact that contour has any thing to do with the deviation of the needle from the normal values. Similar flexures are also seen in the lines of equal inclination and the lines of intensity. One and perhaps two years will be required to accomplish the work properly. There is nothing new in the subject, except the rather unexpected flexures which we found in these lines. It shows very clearly that the isogonic lines which are published for the use of surveyors are of no earthly use. Work ought to be done in a detailed way over the whole country; and I hope we shall some time be able to combine with these determinations a series of magnetic values at ten or twelve different stations in the state of Missouri, and also simultaneous determinations of earth-currents upon lines making angles with each other at the different stations. Similar variations would probably be found in the states of Illinois and Iowa.

In the discussion which followed, President Rowland said, that with respect to the earth-currents, he himself never saw any experiments which gave steady earth-currents. Earth-currents are usually supposed to vary very quickly. They do not pass in a steady direction anywhere; and therefore he would inquire whether Professor Nipher has any reason to suppose there are such earth-currents, and, further, whether these local changes of these lines may not be due to hidden mines of iron, or something or other, rather than to earth-currents.

The question was also asked, whether, in comparing earlier observations with the later, there are variations from year to year which would soon invalidate any survey that could be made, and render it comparatively of no value.

I suppose, replied Professor Nipher, that, over rather large areas of country, the annual change does not vary very rapidly in space. In the western states, so far as I know at present, it is pretty nearly constant, though I do not know as we have any reason to say that it is really constant. Replying to the president's last question, I should say that the determination to which I have referred, as regards earth-currents, was not for the purpose of testing the theory which I formerly had, but simply for the purpose of examining a cause which certainly has some effect. I think it is well enough known that it is a fact, and it is well to investigate it, since we found so many unexpected things. I should suppose that the explanation, that it is due to magnetic matter under the surface of the earth, is the much more probable one, as the case stands now. As to the disposition of that magnetic matter, you can make a great variety out of that, and locate your mines in various parts of the state.

Prof. A. E. Dolbear inquired whether any investigations have been made as to the direction of earth-currents; and whether Professor Nipher knew of any device which would enable him to detect the

direction of them in any place. He had made some observations on a line of his own, half a mile long, and had invariably found that in that line the current is in one direction; and its electro-motive force varies from about one-tenth of a volt up to three volts.

In regard to these lines, said President Rowland, quick flexures of that sort must be due to local causes. They cannot be due to any thing at the centre of the earth. With respect to using a line in determining earth-currents, I think it is unsatisfactory. I do not believe very much in it, myself. You can get a current in the line, but you are not certain it is in the earth.

A member remarked that in 1881, in Boone County, Missouri, he had a line in which a continuous current was evinced with an electro-motive force of from two to four volts. From 8 to 10 in the morning was the maximum, and 5 P.M. the minimum. The line being east and west, the direction of the current was from east to west.

President Rowland said: If you put the wire on the earth's surface from one point to another, you merely determine the difference of intensity between those points. It shows there is a current there when the wire is there, but not when the wire is not there.

A method of distributing weather forecasts by means of railways.

BY T. C. MENDENHALL OF COLUMBUS, OHIO.

THIS system has only been in operation in Ohio for about a year. To distribute forecasts, we place signals upon the sides of the baggage-cars, as distinct as possible from each other, so as to be easily recognized at considerable distances, and also to convey as much meaning as possible, so as to predict as many different conditions. We adopted a combination of form and color. The signals are three in number as to form, and two in number as to color. The red signals are confined to predictions as to temperature, — rise in temperature, stationary temperature, falling temperature. The other color is blue, and that is confined to predictions in regard to the general state of the weather. The question of form was a good deal considered, and three forms were adopted. We adopted the sun, moon, and star, because everybody was familiar with those words. We experimented with the triangle, and finally rejected it. The device for attaching to the car is due to Mr. Anderson, who has been in the service of the board of commissioners for the past year; and it is a really happy device. The signal is made as large as possible, and the disk can be seen a long distance. The red sun and blue moon mean higher temperature and general rain. The crescent means lower temperature; the full disk of blue means general rain; the star represents local rains. With regard to the proper working of the system, though it has been in operation but a short time, it has really done good work. We receive special telegrams every morning, and they are transmitted to the train-despatchers at five o'clock. We are as yet operating it only on one railroad. It happens, fortunately, that that road goes through an agricultural region of considerable importance. It is the road connecting the cities of Columbus and Cleveland. Two trains start out in the morning, at the middle point between those cities. The signals are put on the cars at five o'clock in the morning; and as they run through the morning hours, the farmers along the line can have an opportunity of seeing them, and predicting the weather for the day. The railway company circulated through the whole line little cards, having these signals displayed in colors, with their meaning in every combination. This helps us, because it enables everybody to understand what is meant. A recent communication from Gen. Hazen indicates a disposition on the part of the general government to take hold of the matter, and bring it into general operation as far as possible. Postal-cards have been sent to various persons along the line, with questions in regard to the practical working of the system, which are answered and sent in at the end of every week; and we find, that, on the average, 80 per cent of the predictions are verified.

Plan for a state weather service.

BY F. E. NIPHER OF ST. LOUIS, MO.

WHILE a good many are accommodated by the weather-signals which Professor Mendenhall has already inaugurated, many live a distance from the railroad, and cannot be interested in a scheme which makes it necessary to travel eight or ten miles to learn about the weather, because they might be interested in a different kind of weather by the time they got home. The information might be most easily circulated by telegraphing from picket-stations to the westward. There might be a line of stations on the railroad north and south; and stations might be found necessary in Nebraska, which would give immediate warning to the central office whenever it began to rain at the station; and a code might be arranged, so as to give the idea of the operator as to the probable violence or duration of the rain. Of course it would be necessary to make special study of the general laws for the progress of summer rains. Supposing the information is telegraphed to the central station, the predictions can easily be made out as soon as the picket-stations could be reached, and a clear idea obtained as to the probable direction of the storm, and the time at which it would reach the different portions of the state. That information could be transmitted by the railway companies. Finally, we should make more intimate connection between these and private telegraph-lines which can be constructed by the persons who are to be served with the weather-signals. This plan contemplates the erection of private telegraph-lines leading in from the country to the stations. Upon a twenty-mile line, which would be a frequent length in Missouri, ten farmers will have to pay for the erection of a couple of miles of wire, and the instruments, which can be put up for $30 a mile. Some person could be sent from the vicinity to the director of the service, and instructions given him in regard to the manner of operating the

line and the management of the batteries. The cost of the line, therefore, to each farmer, would be, say, $75, which might be distributed over ten years. Mr. Nipher stated that in several localities the farmers will undertake it just as soon as the information can be furnished them. At the stations the lines could easily be made to terminate in the store of some merchant, who is anxious to secure the trade of the people on the line. This can be done at once in Missouri. The only thing necessary is for the state to appropriate a small amount of money to supply the persons and instruments for observations, rain-gauges, etc. The two things necessary to make it successful are information as to rainfall, and time of beginning and ending of rains.

NOTES AND NEWS.

—The next meeting of the American association for the advancement of science will be held in Philadelphia, probably during the first week in September, 1884. At the session in Minneapolis last Tuesday, the following persons were chosen as officers for the Philadelphia meeting: President: Dr. J. P. Lesley, of Philadelphia. Vice-presidents: Section A (mathematics and astronomy), Prof. H. T. Eddy, of Cincinnati; B (physics), Professor John Trowbridge, of Cambridge; C (chemistry), Prof. J. W. Langley, of Ann Arbor; D (mechanical science), Prof. R. H. Thurston, of Hoboken; E (geology and geography), Prof. N. H. Winchell, of Minneapolis; F (biology), Prof. E. D. Cope, of Philadelphia; G (histology and microscopy), Prof. T. G. Wormley, of Philadelphia; H (anthropology), Prof. E. S. Morse, of Salem; I (economic science and statistics), Hon. John Eaton, of Washington. Permanent secretary: Mr. F. W. Putnam, of Cambridge. General secretary: Dr. Alfred Springer, of Cincinnati. Assistant general secretary: Prof. E. S. Holden, of Madison. Secretaries of the sections: A, Mr. G. W. Hough, of Chicago; B, Mr. N. D. C. Hodges, of Salem; C, Prof. R. B. Warder, of Cincinnati; D, Prof. J. B. Webb, of Ithaca; E, Prof. E. A. Smith, of Tuscaloosa; F, Prof. C. E. Bessey, of Ames; G, Dr. Romyn Hitchcock, of New York; H, Mr. W. H. Holmes, of Washington; I, Mr. Charles W. Smiley, of Washington. Treasurer: Hon. William Lilly, of Mauch Chunk.

—A course of eighteen special lectures will be given next year to members of Johns Hopkins university on topics relating to instruction in the higher institutions of learning. They will be informal lectures, connected only by the general purpose of helping advanced students who are looking forward more or less definitely to the work of teachers to become familiar with the principles and methods followed by other persons, and with the results which have been obtained in different types of educational establishments. The following are announced:—

The present state of university and collegiate instruction in this country, by D. C. Gilman; Recent observations on educational foundations in Europe, by D. C. Gilman; Natural and ethnic history of arithmetic, by J. J. Sylvester; The educational value of grammar, by B. L. Gildersleeve; The future sphere of classical philology, by B. L. Gildersleeve; Educational value of the study of chemistry, by Ira Remsen; What to teach in biology, by H. Newell Martin; One lecture by H. A. Rowland; The observational element in mathematics, by C. S. Peirce; The *a priori* element in physics, by C. S. Peirce; The *naïve* in education, by H. Wood; Modern methods in the study of history, by H. B. Adams; Methods of comparative philology as pursued to-day, by M. Bloomfield; The new impetus given to the study of Latin by the application of the historical method, and by the study of inscriptions, by Minton Warren; Hygiene in collegiate training, by E. M. Hartwell; Rhythm and education, by G. Stanley Hall; The educational value of specialization and original work, by G. Stanley Hall; The uses of libraries in education, by D. C. Gilman.

A course of nine lectures specially designed for college students will also be given, as follows:—

The choice of a profession, by D. C. Gilman; The light which biography throws on college life, by D. C. Gilman; Reading as an auxiliary to study, by W. Hand Browne; The right use of translations, by C. D. Morris; Historical fiction, by H. B. Adams; The English universities, by J. Rendel Harris; Recreation, by E. M. Hartwell; Mental hygiene, by G. Stanley Hall; Science work, by Ira Remsen.

—The Imperial meteorological observatory of Japan has established a telegraphic weather-service, and at present receives reports from twenty-two well-distributed stations. No forecasts are yet attempted, although it is the intention to make them as soon as sufficient experience will justify the step. Tri-daily maps and bulletins are, however, prepared. It is interesting to note that but one telegram is received each day from the several stations. This is sent by the aid of a cipher, which consists of a simple combination of figures, not of words, as is the case in the cipher used by the U. S. signal-service. The daily despatch is the equivalent of about eight words, and contains all the usual meteorological data for each of the three preceding observations.

—The Meteorological council publishes the results of rainfall observations at three hundred and thirty-six stations in Great Britain, made without interruption from 1866 to 1880, under the supervision of Mr. G. J. Symons. The monthly means are given for each year, for each period of five years, and for the whole fifteen years. No discussion of the observations is made, though it would seem that valuable conclusions could be derived from them.

—Mr. V. T. Chambers, an entomologist well known for his studies on the Tineina, died at his residence in Covington, Ky., at two o'clock on the morning of Aug. 7. During the afternoon of Aug. 6 he had a stroke of paralysis, and died from its effects. He was fifty-two years old on that morning. He was a constant contributor to the *Canadian entomologist* and many other entomological journals. In the Bulletin of the U. S. geological survey there are several papers from his pen: viz., the Tineina of Colorado; notes on a collection of tineid moths made in Colo-

rado in 1875 by A. S. Packard, jun.; on the distribution of Tineina in Colorado; new Entomostraca from Colorado; descriptions of new Tineina from Texas, etc.; Tineina and their food-plants; and an index to the described Tineina of the United States and Canada. He also contributed a number of papers to the *Journal* of the Cincinnati society of natural history, of which he was a member, and at one time president. The most important of these papers were: on the tongue (*lingua*) of some Hymenoptera; on Pronuba yuccasella Riley, and the habits of some Tineina; his annual address as president of the society on the metamorphoses of insects, as illustrated in the tineid genus Lithocolletis of Zeller; descriptions of some new Tineina, with notes on a few old species; illustrations of the neuration of the wings of American Tineina; and on the antennae and trophi of lepidopterous larvae. Many of these papers are illustrated by his own drawings. A lawyer by profession, he found time to do much excellent work in science, and formed a large collection, which has been for some years in the Museum of comparative zoölogy at Cambridge. He was also proficient as a microscopist and a botanist. He leaves a wife and three sons, and his loss will also be felt by all the entomologists of the country.

— Dr. John A. Warder, for many years one of the most prominent horticulturists and foresters in the west, died at his home at North Bend, O., on July 14, in the seventy-second year of his age. He has been identified with the west, and especially with Cincinnati, for nearly fifty years. He was president for many years of the Horticultural society, and has written many papers on botanical and kindred subjects. He was one of the founders of the American forestry association, always took an active interest in its proceedings, and contributed many papers to its meetings.

— Professor Simon Newcomb has taken passage for home in the Bothnia, which sails to-morrow from Liverpool to New York. He was to attend the meeting of the French association for the advancement of science at Rouen, just closed. Prof. E. C. Pickering, who has been spending the summer in Europe, will return in October.

— "At the end of May," says Dr. G. Hinrichs in his July Iowa weather bulletin, "this year's growing season, counted from April 1, was sixty degrees in the aggregate ahead of last year's. We had gained nothing more at the end of June; for last year's June was moderate, the same as this season's June. But during July we gained in the aggregate one hundred degrees over last year's July; so that, on the 1st of August of this year, we have received in the aggregate one hundred and sixty degrees of heat more than last year at this period. This fact, together with the fair sky and generally favorable distribution of rainfall, accounts for the greatly superior condition of our crops this year.

"The storm-record," he adds, "has been given in sufficient detail to help to dispel the exaggerated notions of danger from whirlwinds in Iowa. It will readily be seen, that if squalls extending simultaneously over a large storm-front, and progressing for hours like a huge wave, are heralded as 'tornadoes' at every place they reach, people at a distance will soon wonder that towns exist at all in the north-west, and our own people will be scared into expensive tornado insurance. In time our buildings will be substantial enough to withstand our summer squalls and winter blizzards successfully. As to genuine tornadoes, they are rare, and very limited in extent."

— For some months the electricians of Paris have held a monthly dinner. These dinners owed their origin to Count Hallez-d'Arros, and were attended by no organized society, but were re-unions of those interested in electrical science. Lately it has been thought better to give the gatherings more stability by some manner of permanent organization; and at the June meeting a *Société des électriciens* was formed.

— During the past year, original investigations in the following subjects, among others, have been carried on in the physical laboratory of Johns Hopkins university under the direction of Professor Rowland and Dr. Hastings: on the photography of the spectrum by means of the concave grating (the photographs of the spectrum, so far made, extend down to B, the original negatives being about $\frac{2}{3}$ the scale of Angström's map from B to b, equal to Angström's from b to G, and $1\frac{1}{2}$ Angström's from G to the extreme ultra-violet; they show 150 lines between the H lines, and give the 1474 and b_3 and b_4 widely double and the E line indistinctly double); on the determination of the B. A. unit of electrical resistance in absolute measure; the determination of the specific resistance of mercury; the variation of the specific heat of water with the temperature; the relative wave-lengths of the lines of the spectrum by means of the concave grating; the effect of difference of phase in the harmonics on the timbre of sound; and on the variation of the magnetic permeability of nickel by change of temperature.

— Professor Palmieri announces the existence in the lava of Vesuvius of a substance giving the spectrum line of 'helium,' — an element hitherto recognized only in the sun. He considers the late disaster at Ischia to be due to subsidence of land consequent on the unusual activity of Mount Vesuvius.

— There will shortly be published by Allen & Co. of London a book by A. H. Swinton, entitled 'The influence of the sun on natural phenomena.' One may judge of the book's value by the following quotation from the prospectus: "The multitude who read the morning's newspaper may find in it some reason for their successes and losses, further than blind fatality."

RECENT BOOKS AND PAMPHLETS.

Cogniaux, A. Petite flore de Belgique à l'usage des écoles. Mons, *Manceaux*, 1883. 232 p. 12°.

Cock, A. de. Flora der Dendervallei. Analytische sleutel der familien en geslachten (zaadplanten af phanerogamen). Gand, *Meyer-Van Loo*, 1883. 108 p. 8°.

Dandois de Mellet. Du rôle des organismes inférieurs dans les complications des plaies. Bruxelles, 1883. 332 p. 8°.